THURSDAY, APRIL 6, 1871

THE UTILISATION OF NATURAL HISTORY MUSEUMS FOR SCIENTIFIC INSTRUCTION IN GERMANY

IN the following notes we propose to consider the Natural History Museums in Germany, and to see to what extent and in what manner, indirect or direct, they are utilised for the scientific instruction or education of the people. These institutions are very numerous, there being one in nearly every larger German town, and even two or three in places like Vienna and Hamburg. Some comprise collections of zoological, anatomical, palæontological, botanical, and mineralogical objects, while others are limited to one or more of these branches, but whatever their contents may be, we do not recollect visiting one of these Museums in which the objects were left unnamed or unarranged. The majority are State establishments, under the direction of a single head, who is responsible to the Minister of Public Instruction. If the establishment is very extensive, the collections of the various branches are placed under more than one director, the administration and responsibilities being divided. Their development is in some measure dependent on the direct assistance of the Government, but still more on the energy and capability of the director, inasmuch as, of two Museums originally supported by the same grant of money, one has remained stationary for years, whilst the director of the other, making the best use of his independent position, has known how to raise the value of his Museum as a purely scientific or instructive institution, thus establishing claims for additional assistance, which could not be neglected by the Government.

We may divide the German Museums into—(1) Those founded with the intention of exhibiting objects of Natural History to the general public; and (2) those established for educational purposes.

There are not many of the former class. To it belong the Museums of the formerly independent "Reichsstädte," Hamburg, Bremen, and Frankfort-on-the-Main, one of the Vienna Museums, and the collections in Stuttgart and Darmstadt. There are others, like that in Mayence, but they have more the character of well-arranged local country museums. Although originally founded for the purpose of exhibiting curiosities, they soon took another position by receiving objects in which the general public takes a very limited interest (as, for instance, botanical, geological, or mineralogical specimens), and by systematically collecting materials for the purposes of purely scientific research. In several instances the scientific results were sufficiently important and extensive to form not only a nucleus but the sole subject-matter of distinct periodical works, such as the "Annalen des Wiener Museums," the "Museum Senckenbergianum," the "Abhandlungen des Hamburger Museums." The Frankfort Museum became the head-quarters for the Zoology of North-eastern Africa; Bremen possesses a unique collection of African birds, celebrated not only for the great number of standard specimens, but also for their beautiful state of preservation. In the Vienna Museum particular attention was paid to European fresh-water fishes; and travellers like Natterer, Russeger, Kotschy, enriched it with collections so numerous that the Austrian Naturalists have been engaged in their examination till within a very recent period. The Stuttgart and Darmstadt Museums are now celebrated for their valuable collections of South-German fossils, worked out by G. von Jäger, Kaup, and others.

In the Museums of this class great attention is paid to the local Flora and Fauna, recent and extinct. Thus the Stuttgart collection may be mentioned as a model of what a Museum ought to be; besides a most complete series of the plants and fossils, it contains a collection of the animals of Suabia in all stages of growth and development and of variation, in a perfect state of preservation, and particularly attractive from the life-like manner in which the specimens are mounted.

It was the natural consequence of the growth of a Museum, especially in the smaller of the towns mentioned, that it became a conspicuous object; that, although opened to the general public on certain hours of Sunday only, the crowds of visitors increased; that the interest in it spread into wider circles; and that, more or less in connection with it, societies were formed whose aim was the distribution of knowledge, and which were accessible to all. Although no great benefit is individually derived by the majority of the members of such societies from their rather superficial connection with Science, yet we ought to remember that even the fragments of knowledge picked up by them have the effect of expanding their ideas. A community of feeling grows up between the professed naturalist and the uninitiated, by which the former is encouraged in his further pursuits, and induced to consult in his labours and communications the requirements and understanding of the latter. Some of the German Governments, especially the Austrian, have also acknowledged the value of such societies by granting them an annual subsidy.

One out of several instances may be brought forward in evidence of the correctness of the above remarks, viz. the example of the town of Frankfort, to which many years ago a small miscellaneous collection and library was bequeathed, under the name of "Museum Senckenbergianum." By the able management and disinterestedness of Dr. Eduard Rüppell, it grew into a considerable zoological collection, of which the inhabitants were justly proud. An interest in Natural History being awakened, a body of men, chiefly wealthy merchants, formed themselves into a society, founding a Zoological Garden, and in connection with it a monthly periodical (noticed in one of our previous numbers), in which, at first, only objects of local interest were discussed, but which now has among its contributors eminent naturalists as well as amateurs, and does more for the distribution of sound natural history knowledge among the general German public than all the other scientific periodicals put together.

While thus we fully acknowledge the value of the Museums of this class as offering materials for really scientific original research, and as creating and maintaining an interest in Science among the general public, we understand that they are not utilised in a direct manner for methodical scientific instruction. Although Natural History is taught in nearly all the so-called Gymnasiums and Polytechnic Schools, the time devoted to it is limited, and those Museums are only visited at intervals by the classes under the guidance of teachers.

THE DESCENT OF MAN

The Descent of Man, and Selection in relation to Sex. By Charles Darwin, M.A., F.R.S., &c. In two volumes. Pp. 428, 475. (Murray, 1871.)

I.

IF Mr. Darwin had closed his rich series of contributions to Science by the publication of the "Origin of Species," he would have made an epoch in Natural History like that which Socrates made in philosophy, or Harvey in medicine. The theory identified with his name has stimulated ethnological and anatomical inquiries in every direction; it has been largely adopted and followed out by naturalists in this country and America, but most of all in the great work-room of modern science, whence a complete literature on "Darwinismus" has sprung up, and there disciples have appeared who stand in the same relation to their master as Muntzer and the Anabaptists did to Luther. Like most great advances in knowledge, the theory of Evolution found everything ripe for it. This is shown by the well-known fact that Mr. Wallace arrived at the same conclusion as to the origin of species while working in the Eastern Archipelago, and scarcely less so by the manner in which the theory has been worked out by men so distinguished as Mr. Herbert Spencer and Prof. Haeckel. But it was known when the "Origin of Species" was published, that instead of being the mere brilliant hypothesis of a man of genius, of which the proofs were to be furnished and the fruits gathered in by his successors, it was really only a summary of opinions based upon the most extensive and long-continued researches. Its author did not simply open a new province for future travellers to explore, he had already surveyed it himself, and the present volumes show him still at the head of his followers. They are written in a more popular style than those on "Animals and Plants under Domestication," as they deal with subjects of more general interest; but all the great qualities of industry and accuracy in research, of fertility in framing hypotheses, and of impartiality in judgment, are as apparent in this as in Mr. Darwin's previous works. To one who bears in mind the too frequent tone of the controversies these works have excited, the turgid rhetoric and ignorant presumption of those "who are not of his school —or any school," and the still more lamentable bad taste which mars the writings of Vogt and even occasionally of Haeckel, it is very admirable to see the calmness and moderation (for which philosophical would be too low an epithet) with which the author handles his subject. If prejudice can be conciliated, it will surely be by a book like this.

It consists of two parts. The first treats of the origin of man, his affinities to other animals, and the formation of the races (or sub-species) of the human family. Besides the obvious interest to all Mr. Darwin's readers of a discussion on the subject of their "proper knowledge," naturalists will find the detailed application of the laws of natural selection to a single common and well-known species an excellent test of their truth and illustration of their difficulties. It is in dealing with the latter, which are never extenuated or passed by, that the author introduces the subject of sexual selection. This is dealt with in the second part, which forms more than two-thirds of the work, and that not only as it affects man, but in its entire range. Reserving this division of the book for a future article, we will endeavour here to give a summary of the course of argument in the earlier portion.

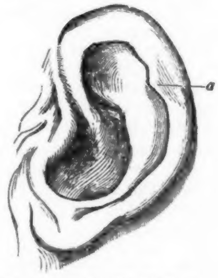
The author, justly assuming that the general principles of natural selection are admitted by all who have examined the evidence on the subject, with the exception of many of "the older and honoured chiefs in natural science," proceeds at once to discuss the proofs of the origin of man considered apart from those affecting all animals in common. The first group of facts adduced to show his kinship with other forms of animal life, relate to the strict correspondence of his bodily parts with those of other mammalia. To say that these structures are the same because they have the same uses, is untrue, for many of them have no use in the sense of active function, and we constantly find the same structures in animals turned to different uses, and the same uses subserved by different structures. To say that the bodies of men and animals are alike because they are formed on the same plan, or because they are the realisation of the same idea in the Creator, is true enough, but is beside the mark; for natural science inquires how or by what steps these things have become so, not why and from what first cause. If one sees two men very much alike, one naturally supposes that they are brothers; if they are rather less so, they may be cousins; if only agreeing in general characters, we recognise them as at least belonging to the same race or nation; and so, when the facts to be accounted for are once ascertained, nothing but prejudice or repugnance to acknowledge our true relations, can explain why it was so long before naturalists admitted the hypothesis of community of origin between men and other animals. What is called the Darwinian theory accounts for the way in which diversities have arisen, and thus has converted an apparently obvious hypothesis into a well-grounded theory. But in expounding the likeness between men and animals, the author does not confine himself to anatomical structure, but shows how the same resemblance extends to the laws of disease, the distribution of parasites, and other minute particulars.

The next argument brought forward is the equally familiar one drawn from the likeness of the human embryo to that of other vertebrata. Then follows an account of the rudimentary organs in man, which in all other species are justly held among the most important indications of affinities. One such rudiment is mentioned which is, we believe, hitherto unrecorded. It is a slight projection of the rim of the helix of the auricle, which would correspond when unfolded to the point of an erect ear. (See illustration.) This occasional abnormity may, perhaps, be recognised by future anatomists as the *Angulus Woolnerii* after its first observer.

In the second chapter Mr. Darwin shows that a consideration of the mental faculties of man, including the use of language, which has been held the greatest difficulty to admitting his kinship to other animals, may rather strengthen than weaken the arguments derived from his bodily structure. Memory and curiosity, jealousy and friendship, and even the power of correct reasoning, and of communication by sounds, are shown to belong to many of the lower animals, while the faculty of reflection and self-consciousness, and "the ennobling belief in the existence of an Omnipotent God," cannot be ascribed to the lowest tribes of the human family. At the same time

it is argued that the use of articulate language, the power of forming abstract ideas, and even the sense of right and wrong, may have been gradually acquired by steps which here and there it is not impossible to trace. The question of the origin of the moral sense leads to the proposition of the following theory. Some natural emotions are of great intensity but short endurance, and their force is not easily recalled by memory; others, though less powerful at certain times, exert a constant influence, or one which is only interrupted by being overpowered for a time by the former. Accordingly, during the greater part of life, and always when there is leisure for reflection, the gratification connected with the more violent passions, such as hunger, sexual desire, and revenge, appears small, whereas the social instincts of sympathy and the pleasures of benevolence exert their full power. Hence we find social virtues, as courage, fidelity, obedience, among savages and even animals, long before the "self-regarding" virtues begin to appear. This theory is analogous to that by which Mr. Bain explains the higher character of the pleasures of sight compared with those of smell; they can be more easily recalled ;and corresponds to the distinction drawn by the same writer between the acute and the more "massive" and permanent pleasures.

In the fourth chapter Mr. Darwin discusses the manner in which man was developed. It is shown that the broad facts on which the theory of Natural Selection rests apply



Human Ear, Modelled and Drawn by Mr. Woolner. *a*. The projecting point

to him. He is prolific enough to share in the struggle for existence. In him, as in all organic forms, there is a constant tendency to growth, which being checked and modified by external influences, proceeds in the direction of least resistance, and so produces the variations which are often ascribed to an assumed inherent tendency. Among the various forms produced, those will survive which are best fitted for the surrounding conditions, and they will transmit their character to their descendants, still subject to the same liability to vary. Next the author argues that the mental endowments of man, including language, his social habits, his upright position, and perfect hands, are of direct advantage to him in the struggle with other animals and with his fellows. It has always appeared that the difficult point in the development of man by Natural Selection is at the period when he was more defenceless than an anthropoid ape and less intelligent than the lowest savage; but Mr. Darwin thinks that the transition may have been safely made in some large tropical island where there was abundance of forest and of fruit. That man, once developed, can maintain himself, is obvious from his present existence. The arguments in favour of civilised man being the descendant of savages, which have been so admirably developed by Sir John Lubbock and Mr. Tylor, are of course brought forward in support of the author's view, and the important question is discussed how far we may hope for future improvement in the race by means of continued Natural Selection. Thus, while admitting that the process undergoes many checks and complications among human beings, the author does not assent to the arguments urged by Mr. Wallace that it would cease to operate as soon as the moral faculties came into play.* One human peculiarity which is apparently inexplicable by Natural Selection, the nakedness of the body and presence of a beard, is referred by Mr. Darwin to the operation of Sexual Selection. To this same agency is attributed the origin of the so-called Races of Man, which is discussed with admirable clearness and impartiality in the last chapter, and this leads to the complete exposition of the theory of Sexual Selection which occupies the second part of this work, and must be considered in a future article.

It only remains here to add a word on the account of the affinities and genealogy of man contained in the sixth chapter. As a kind of retribution for the attempt to raise Cuvier's order *Bimana* into a sub-class, not only have most naturalists now reverted to a modified definition of the *Primates* of Linnæus, but Mr. Darwin shows reasons for refusing to the genus *Homo* even the rank of a family in this order, which Prof. Huxley admits, and regards it simply as an aberrant member of the Catarrhine division of the *Simiadæ*. This conclusion, which seems to us to be a just one, will only be distasteful to those who so little appreciate the true characters of man as a spiritual being, that they could feel self-complacency in the brevet-rank of a sub class.

Mr. Darwin mentions Africa as the possible seat of the Catarrhine progenitors of man, but shows the futility of speculations on this point, until we know more of the recent changes of the earth, the records of palæontology, and the laws affecting the rapidity of animal modifications. He does not advert to Prof. Haeckel's hypothesis of a "Lemuria" in the Indian Ocean, but agrees with him in next tracing the phylum of man to the *Prosimiæ*. These again were developed from "forms standing very low in the deciduate mammalian series" (possibly, as Prof. Huxley suggests, most nearly allied to the existing *Insectivora*), and thus, through the Marsupials and Monotremes from the Reptilian stock, and thence through the *Dipnoi* and Ganoids from the *Urtyhus* of the vertebrate series, represented by the Lancelet alone. Nor does Mr. Darwin stop here, but adds the weight of his judgment to the theory based on the observations of Kowalewsky and Kuppfer, which deduces the primeval *Vertebrata* from a form resembling a Tunicate larva. Perhaps the most brilliant of the many new suggestions in these volumes is one thrown out incidentally in a note to p. 212, and based upon this supposed relation of man to the Ascidians. Beyond the organic world Mr. Darwin does not attempt to trace the genealogy of man. Considering how essential this extension of the theory of evolution is held by men so distinguished as Haeckel, and how keenly the question

* In reviewing in these columns the contributions of the latter eminent writer, we took occasion to quote the estimate he expresses of Mr. Darwin's claims. Should anyone be disposed to overlook the original value of Mr. Wallace's work, he will be corrected by a somewhat similar passage in the present volume. See pp. 137, note, and 416.

of Abiogenesis has recently been discussed, the reticence shown in avoiding allusion to the subject is perhaps the most remarkable among the many remarkable characters of this great work. P. H. PYE-SMITH

OUR BOOK SHELF

Elementary Natural Philosophy. Being a course of nine lectures by J. Clifton Ward, F.G.S., Associate of the School of Mines. (London: Trübner and Co.)

THE attempt to crowd the Elements of Natural Philosophy into nine lectures cannot be otherwise than a failure. This is signally the case with the little book before us. We need hardly go farther than the table of contents to justify the statement. A single lecture is devoted respectively to Magnetism, Voltaic Electricity, Light, and Heat; Pneumatics and Hydrostatics together occupy one lecture, whereas to Frictional Electricity and Sound are given two lectures apiece. Nor does the author confine himself to a simple summary of the leading facts in each of these subjects, he tries to rush over all the field occupied by larger text-books. Hence, important facts are often lightly passed over and comparatively trivial matters made unduly prominent. In Voltaic Electricity, for example, two pages are occupied with a description of the effects of electro-chemical decomposition, when seen on the screen by the aid of the solar microscope. We recognise here, and indeed on every page of the book, those lecture-experiments with w ich Dr. Tyndall has made the students of the School of Mines so familiar. Mr. Ward has not only drawn largely upon his notes of those lectures, but he imitates Dr. Tyndall's language and style.

Notwithstanding this, we are quite sure Mr. Ward has only himself to blame for the errors which even a cursory glance has revealed to us. On p. 85 we read "Magnetism may be produced by *friction* (of *soft iron* with loadstone or other magnet) by magnetic induction and electricity." Magnetism is *not* produced by friction of soft iron. On pp. 36 and 37 Mr. Ward has fallen into a vulgar and serious error in explaining the electric wind. Speaking of the so-called electric fish, here is what he says:—"If the interior of the Leyden jar be charged positively, negative electricity will be attracted to the head of the fish, from the somewhat blunt point of which it will stream and cause a movement from the knob; while the gliding off of the repelled positive from the finer pointed tail, will counterbalance this movement, and keep the body in equilibrium." The author also speaks of a lighted candle extinguished by the *draft of electricity* streaming from a point. This, of course, is grossly incorrect; it is the movement of contiguous air particles charged similarly by contact and then repelled, that extinguishes the candle, or supports the gold leaf fish.

Though there are some good points in this little book, we regret our inability to recommend it either to schools or students. We venture to think the author betrays his want of experience in teaching science by the over-crowding of his facts; the first lecture, for instance, is accompanied by thirty-three distinct experiments. Teaching—especially science teaching—requires "precept upon precept, line upon line, here a little and there a little:" otherwise there is an almost certain danger of the learner obtaining loose and superficial knowledge, the end of which is not sound instruction, but disastrous conceit.

W. F. B.

Essays on Darwinism. By J. R. R. Stebbing. (Longmans and Co., 1871.)

MR. DARWIN, in his recent work, very truly observes that "false facts are highly injurious to the progress of science, for they often long endure; but that false theories are comparatively innocuous." Mr. Stebbing's work can then do little harm, as it supplies us with no new "facts" whatever, whether true or false. The author is an advocate who serves Mr. Darwin with more zeal than discretion, and who seems but little, if at all, able to appreciate the arguments and objections adduced on the other side. Some who are already convinced of the truth of Darwinism will read with pleasure a series of eloquent and interesting essays in its favour; but, though calculated to confirm a disciple, they are singularly ill-calculated to convert an opponent. Before Mr. Stebbing again writes upon this subject we strongly recommend him to peruse carefully Mr. Grote's "Examination of the Utilitarian Philosophy."

Das Wesen und die Ziele der Chemischen Forschung und des Chemischen Studiums. Akademische Antrittsrede gehalten von Dr. Rudolph Fittig. (Leipzig: Quandt and Händel, 1870. London: Williams and Norgate).

SO busy are the majority of German chemists in research, that it is seldom we are privileged to have their opinions on the object of the science, and the position it should occupy as a study. Dr. Fittig has availed himself of his appointment as Professor of Chemistry in the University of Tubingen to deliver an inaugural address, in which these points are discussed with great clearness and ability. Starting with the assumption that the majority of men estimate the value of a science only by its power to satisfy want and contribute to the comfort of life, Dr. Fittig goes on to claim for chemistry from this point of view the first place among the sciences. "Where," he asks, "is there another science which, in the application of its results to man, almost from his first breath to his last, is so true a companion as chemistry?" and he proceeds to show that it is useful, not so much in explaining what the nourishing constituents of food are, as in disclosing the laws of agriculture, and thus teaching us how to produce means of nourishment. Further, he points out that there is not an article of clothing for the preparation of which chemical knowledge has not been employed, and the same knowledge is necessary to show how the spread of disease may be prevented, and cured when it has taken hold. While these practical results are obtained by the study of chemistry, Dr. Fittig is careful to show that it is a total misunderstanding to suppose that its chief purpose is to discover brilliant colours or new medicines. Thus, without undervaluing the practical importance of the discovery of the aniline colours, it is nevertheless true that the splendid results obtained by Hofmann would have had the same interest for the chemist, had these compounds been colourless and without any technical use. So we are told, "The task of chemistry is to explain the composition of bodies and all phenomena resulting from change of this composition *in order to derive the regular connection and cause of these phenomena, and therefore also of the natural laws which regulate the building up and decomposing of substances.* . . . . We are compelled to multiply the number of substances already existing in nature, not for the sake of producing new bodies and benefiting the world, *but to discover the eternal laws of nature.* He is no true chemist who only prepares new compounds without any definite aim (although, perhaps, he has prepared a large number of compounds hitherto unknown and possibly very beautiful in appearance), and his work has no direct value for science, and can only become valuable when employed by others in its true scientific sense. . . . . True scientific researches must never be given over to chance, they must be systematically planned, begun with a clear consciousness of what is to be attained, and finished in the same spirit." Dr. Fittig has done well to point out so clearly the true aim of the science of chemistry, and to disparage the false estimation of its value, which would make it simply a means of discovering bodies with some technical or useful application. And even in this direction, which must

always be looked upon as of secondary importance, we are convinced that greater progress will be made if chemistry is regarded and studied from the high point of view so forcibly pointed out by Dr. Fittig in his interesting address. F. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Chemical Research in England

IN confirmation of your remark in the last number of NATURE that, in regard to scientific discovery, we in this country "are conspicuous for our prominent position in the rear," will you permit me to state the result of an inquiry which I lately made into the comparative activity of this and other countries in the prosecution of chemical research?

In the year 1866 there were published 1,273 papers on new discoveries, by 805 chemists, 1·58 paper being thus the average produce of each investigator. Of these, Germany contributed 445 authors and 777 papers, or 1·75 paper to each author; France, 170 authors, and 245 papers, or 1·44 paper to each author; whilst the United Kingdom furnished only 97 authors, and 127 papers, or 1·31 paper to each author; all other countries yielding 93 authors and 124 papers, or 1·33 paper to each author. Thus, not only are we far behind in the aggregate of activity in discovery, but our individual productiveness is also markedly below that of Germany and France. From a purely national point of view, our case is even worse than it appears to be from a comparison of these figures, since a considerable proportion of the papers contributed by the United Kingdom were the work of chemists born and educated in Germany.

It will be seen that the above comparison covers chemistry only; but there is every reason to believe that in other sciences, the progress of which depends, like that of chemistry, upon experimental investigation, our position is still worse. It is highly remarkable that a country which, perhaps more than any other, owes its greatness to the discoveries of experimental science, should be distinguished for its neglect of experimental research. But the causes of this anomaly are sufficiently obvious; they are:

1st. The want of suitable buildings and apparatus for the prosecution of such investigations.

2nd. The non-recognition of experimental research by any of our universities.

With regard to the first of these causes, the prosecution of experimental discovery in this country is rendered extremely difficult, if not impossible, to those who do not possess ample private fortunes; and even to such as have this advantage, it is by no means easy. A laboratory of research is not a convenient or agreeable adjunct to a dwelling-house, and it is generally prohibited by the terms of the lease or covenant; indeed it is agreed on all hands that most of the operations which are required for the prosecution of inquiries in chemistry, physics, and physiology, ought only to be carried on in buildings specially devoted to the purpose. But where are such buildings to be found? Our chemical laboratories are only adapted for beginners, there is not in any one of them a separate department constructed and fitted for original research. Still less is this the case in physical and physiological laboratories; indeed until Sir William Thomson instituted one in Glasgow some three or four years ago, there was not in the United Kingdom a physical laboratory even for beginners. In Germany, on the other hand, the noble State laboratories of Berlin, Leipzig, Bonn, Heidelberg, Königsberg, and Stuttgart, are provided with special departments where the experimental investigator finds ample convenience and the necessary but costly instruments of precision provided for his use, the payment of a moderate fee only being required to secure all these advantages.

With regard to the second case, the highest degrees, and even honours in experimental science, are given in all our universities without any proof being required that the candidate possesses the capacity to conduct an original experimental investigation, or that he is competent to extend the boundaries of his science. On the other hand, in all the Prussian Universities, and in the best German Universities generally, no candidate is even admitted to examination for his degree unless he first submits to the senate a dissertation on some original experimental investigation conducted by himself. This investigation must also have a sufficient importance; for, as a matter of fact, more candidates are rejected on the ground of insufficiency of dissertation than through failure to pass the subsequent examinations. The entire ignoring of research in the granting of degrees in this country not only effectually prevents the training of students in experimental investigation, and the actual execution of researches by students; but it has also a direct tendency to divert the attention of professors and teachers from original research—they are not called upon to devise, as is the case in Germany, suitable subjects for research to be pursued by their students; and thus, not only is their attention withdrawn from this all-important field of experimental science, but, as their students have to be trained for subjects which are foreign to research, they feel that to devote any considerable portion of their own time to it would be to that extent to neglect their class duties.

E. FRANKLAND

Dublin Observatory

IN an account of the Observatory of Trinity College, Dublin, given in NATURE of March 16, 1871, there is a slight mistake in the date of the erection of the Transit Instrument, which is there assigned to 1808. It was erected many years before; for in the second volume of the Transactions of the Royal Irish Academy, Dr. Usher describes observations made with it in 1785.

I should not have thought the correction of this error necessary but for the fact that this transit marked the epoch of a most important improvement in astronomical instruments. It was the first in which the illumination of wires was effected through the axis by an internal reflector. This invention is described by Usher in the volume already referred to.

1808 was the date of the circle's erection; it having been ordered in 1783. This delay was in one respect fortunate. Ramsden, having quarrelled with Usher, resolved that the latter should never have the circle. On Usher's death Ramsden set to work to complete it, but found, to his dismay, that the extremities of its radial arms had become "rotten," having been acted on by the sulphurous atmosphere of London.

As originally constructed, it was ten feet diameter. He removed the rims (which, I believe, had been also acted on), cut away about six inches from each of the arms, and found the remainder sound. But as he was doubtful about its permanence, he let it lie several years longer, and found his apprehensions verified. He cut off six inches more from each arm and awaited the result, notwithstanding the urgent expostulations of Brinkley; and it was not until a short period before his death that he was satisfied that no farther change was probable. He then completed it at its reduced diameter of eight feet. But it was not divided till after his death (by Berge, his successor).

It is not easy to explain why this destruction was confined to the ends of the arms. To judge from the analogy of the Palermo Circle, the diameter of these arms at the outer extremity was very small; and if they were of cast brass, the molecular condition of the metal there, in consequence of the more rapid cooling, may have been different from that of the more massive portions.

A still more remarkable instance of this destructive action occurred to a circle described by Mr. Bond in the Philosophical Transactions, 1806, and known as the Westbury Circle. This was ultimately established at the old Observatory of the Glasgow University, and in an atmosphere still more sulphurous than that of London. When this University was broken up, and its instruments sold, this circle was purchased by the late Sir James

South. But on its arrival at his Observatory it was found to have suffered so much that it actually fell to pieces! Only a few of the more massive parts were entire; and of the rims of the circle nothing remained except that which carried the divisions, which, as I was informed by Troughton, was of "Dutch brass," and was quite unchanged.

The excellence of this Dutch brass is, I believe, recognised also by watchmakers, and it seems to deserve inquiry to what its superiority over English brass is to be attributed.

It is worthy to be mentioned that among the instruments ordered from Ramsden by Usher was an equatorial telescope driven by clockwork. But owing to Ramsden's feud with Usher, this was not executed; and this important aid to the astronomer, which had been proposed by Hook nearly a century before, lay dormant till it was applied by Frauenhofer, forty years later, to the Dorpat telescope. T. R. R.

Armagh

Morell's Geometry

In answer to your criticisms on my work, "The Essentials of Geometry," I have given an explanation as to the sources of that book. I proceed to give very briefly my arguments for what is there advanced.

I shall do so under two heads, which will be in the form of indictments against my reviewer.

For I charge him, first, with overlooking the nature and object of the book; and, secondly, with overlooking the context of passages he criticises, an omission that changes the entire aspect of the case.

First it is explained (Preface, p. vii.) that my little volume is an attempt at a manual and memento for students, of which so many exist in France and Germany It is notorious that such works do not dispense with others, or touch the plan of school text-books, and then much of their contents consists of results with hints of demonstrations. Then at p. viii. of Preface it is added that an inspection of the methods employed will show that German and French geometricians . . do not condemn the student to keep a geometrical figure rigidly in the place in which it is laid down on paper. Revolution and superposition are allowed . . . simplifying and shortening the proof, &c.

Passing to the second indictment, I find that the criticism levelled against my so-called theorems of parallels commits two serious offences. First, it garbles my matter; secondly, it overlooks the principles first laid down in the preface, applying from logic two propositions treated by me both manually and logically; thirdly, it overlooks claims in pp. 15, 24, 25, where the technical terms in p. 20 are explained; lastly, it ignores the fact that all our theorems about parallels rest on assumptions and not on logic.

It is evident that if superposition be allowed, two parallels as they cannot cut (hyp.) must coincide, thus the angles they form with a secant will be equal.

Again, the charge of want of logic in my proof of the inequality of triangles with three equal sides falls to the ground, if the first clause of p. 44 be read. For as this is a case of superposition by making the bases coincide, the arms of both triangles must coincide as radii of equal length intersecting at only one point on the same side of the base. The proof is equally direct from symmetry, from inversion and juxtaposition, and from subposition, as in the Notes to Todhunter's School Euclid.

As to the critic's difficulties about explaining the coincidence of two semicircles, to any one used to the free handling of geometrical figures in France and Germany, the thing wears a ludicrous aspect.

Then about the statement that two equal adjacent dihedral angles are right angles, a moment's reflection shows that if a dihedral angle be defined (Bos. p. 32) as an angle formed by the revolution of a movable plane at its common section over a stationary plane, when it reaches the point where the two adjacent angles are equal, they must be right angles. The definition may be disputed, but the conclusion is correct.

As regards the criticisms on my definitions, I do not think it necessary to enter into this matter. The ground of definitions is a wide and a disputed one, and I am content to err, if err I do, with very high authorities. Euclid has defined a straight line to be that which lies evenly or equally between its extreme points. This definition affords no assistance in arriving at the properties of straight lines. In Dr. Simson's edition, a point is defined to be that which has no parts or no magnitude. This is objectionable, as being wholly negative. Again Dr. Simson, in the notes to his edition of Euclid, admits that the 11th axiom is not self-evident.

In conclusion, the reviewer is of course as likely to attack the free treatment of theorems and problems practised, especially in Germany, by the conception of the generation of all figures from their elements by the movement of points and lines. But it can scarcely be charged against the author of the "Essentials" that he has not shown some of these shorter methods of demonstration as used in France and Germany.

The nature of revolution is fully illustrated at pp. 6, 8, and the treatment of angles as ratios in note 2, p. 9. Our limits prevented anything more than indications, but *verbum sat.* to the great logicians of England. J. R. Morell

On the Derivation of the name "Britain"

Having been from home, I did not see the letter of "A. H." in your publication of March 16 until yesterday. His only objection to my derivation of the name *Britain* is that the word *tin* in his opinion was "not used in this island so early as the argument for its forming part of the word *Britain* requires. The following remarks will show that it *must* have been used in this island quite as early as the word *Britain*.

His assertion that "our word *tin* is of comparatively modern formation," cannot be established. It *must* have been familiar to the Cornish centuries before Diodorus Siculus described St. Michael's Mount, in Cornwall, under its name of *Iktin*, from whence tin was exported by the Phœnicians as far back as the time of Moses (Numb. xxxi. 22); and from none but the Phœnicians could the Cornish have derived the word *tin*—for that metal, as well as its name, was unknown to them before they were visited by the Phœnicians. The name *Iktin* (Tin-port) by which the Mount was called in the time of Diodorus, proves the existence of the word *tin* prior to that period, and the present Cornish word *stean* can only be a corruption of the very anciently adopted word *tin*—a corruption arising probably during the Roman period, so that instead of *tin* being a corruption of the Welsh *ystaen*, or of the Latin *stannum*, as "A. H." imagines, the reverse is evidently the case.

Assuming, with most authors, the original Phœnician word to be *tin*, that name continues unchanged in the Saxon, English, Dutch, Danish, and Icelandic languages; but the Swedish name is now *tinn;* the German, *zinn;* the French, *étain* and *tain;* the Latin, *stannum;* the Italian, *stagno;* the Spanish *estano;* the Portuguese, *estanho;* the Irish, *stan;* the Welsh, *ystaen;* the Cornish, *stean;* the Armoric, *stean* and also *staen*—the initial letter or sound *s* in each of the last nine names being a mere prefix, as in the modern word *sneeze* for *neeze* (Job xli. 18). With this exception, and except the ordinary terminations of the Latin, Italian, Spanish, and Portuguese names, these thirteen different spellings are merely the different ways in which different nations of Europe pronounce the Phœnician word *tin*.

Diodorus speaks of *Iktin* as an island adjoining *Britain;* and this island (for it *is* an island two-thirds of the day) was no doubt long before his time called sometimes *Iktin* and sometimes *Bretin;*—*Iktin* when it was regarded as a "port," and *Bretin* when regarded as a "mount"—*ik* being the Cornish for "port," and *bre* the Cornish for "mount." It was however most generally known as a mount, and as the most remarkable object in *Mount's* Bay, to which it has therefore given its *English* name, having long before the Christian era, in all probability, given its ancient Cornish name of *Bretin* to the island in which we live.

Plymouth, March 29 Richard Edmonds

Records of European Research

The Chemical Society has taken up a good cause, that of reporting foreign labours much more fully than could be worth the while for any periodical publication to undertake.

I have suggested in another quarter the advisability, if possible, of joining in this scheme. But funds are requisite to have the work well done.

It has struck me that, as a matter of completeness and economy, it would be far better if the learned societies subscribed, and the Royal Society made a grant besides its subscription, to engage an efficient staff to report foreign contributions not merely to one branch of science, but to all, forming, let us say, a quarterly *comptes rendus* of European research.

The practice of quoting the titles of foreign papers is aggravating in the extreme, especially when accompanied by the remark that "this laborious but lengthy paper is not suited for 'abstraction!'"

I seldom go out of our foggy little island without hearing our want of familiarity with what is going on elsewhere roundly and deservedly abused. But I feel bound to say that, more especially in France, I can justly retaliate.

Russian science is said to be very productive. That language, at all events, is beyond most of us.

New University Club MARSHALL HALL

Aurora Australis

OBSERVING in NATURE of Oct. 27, an account of a brilliant aurora observed in England on the 23rd to the 25th of that month, I think it may excite attention to the subject by informing your readers that at the same date a splendid display of Aurora Australis was recorded at most of the meteorological stations in this colony.

The extent of sky covered was not so great as in the fine display on 5th April last, which was also coincident with a remarkable Aurora Borealis, but the red tint was so much deeper than usual, that many persons in this place attributed the phenomenon to the glare of the great fire which destroyed the town of Lyttelton, and the news of which was just then received by telegraph. JAMES HECTOR

Colonial Museum and Observatory, Wellington, N.Z., Jan. 2

Ocean Currents

MY rejection of the idea that permanent differences of atmospheric pressure could produce any effect on Ocean Currents, was meant to be as sweeping as Mr. Johnston considers it. I believe that the idea is repugnant to the most elementary conceptions of hydrostatic equilibrium; and I am particular in so far repeating the gist of my former letters, because Mr. Johnston, in his letter in NATURE for March 9, reiterates his suggestion that difference of atmospheric pressure is a power in the production of Ocean Streams, and whether he suggests that it is a supplementary power, or a chief one, is nothing to the purpose, if, as I distinctly maintain, it is not a power at all.

My rejection of the idea that the formation of these differences of pressure can produce any appreciable effect, is quite as decided; but Mr. Johnston is mistaken when he speaks of my denying also the influence of the movement of these differences of pressure; for my remarks concerning them were to the very opposite purport; and I pointed out that such movements do sometimes give rise to rapid and dangerous sets, known as storm-currents, which in their irregular and exceptional nature, differ essentially from those regular permanent or periodic currents usually understood by the general term Ocean Currents, though they may occasionally modify them both in direction and velocity.

I would also call Mr. Johnston's attention to the fourth paragraph of his letter, and assure him that I have never, directly or indirectly, maintained that the Trade Winds "would account for the whole of the phenomena of Ocean Currents;" but I have maintained, and do still maintain, that all the phenomena alluded to may be very satisfactorily accounted for by a reference to the prevailing winds of the different parts of the world; and that the Gibraltar Current is to be attributed, not to the local, partial, and peculiar wind of the Straits, but to the great body of the west wind of the North Atlantic, which also produces a northerly current on the coast of France, known distinctively as Rennell's Current, and a southerly current on the coast of Portugal.

I have discussed this question so fully in another place, that I should be only repeating myself were I to say more about it here; but I may add that though, as Mr. Johnston asserts, under-currents cannot be caused primarily by the action of the winds, they can be, and frequently are, caused secondarily by that action; and many a ship has owed her safety from the apparently imminent danger of a lee-shore, to the "under-tow," or reflux of the water swept towards the shore on the surface. If there is a deep-flowing outward current in the Straits of Gibraltar, I believe it to be exactly of the nature of an "under-tow;" it seems to me probable enough that there is occasionally such an outward current; but I cannot admit that the *one* observation of it which Dr. Carpenter considers he obtained, after several attempts made in vain, has abundantly proved its existence; still less can I admit that it is necessary to call in difference of temperature and density to account for it.

Mr. Croll considers that there is a similar escape of water, underneath, from the northern to the southern hemisphere, and his arguments appear to warrant the suggestion, although no such under-current, or system of under-currents, has yet been observed. I see no improbability in the idea; but so many mistakes have, at different times, been made by trusting rather to theory than to positive evidence of fact, that we cannot be too cautious in admitting the existence of such under-currents, without any reliable observations. For that recorded by Captain Maury, to which Mr. Johnston refers, has, from the vague manner in which it is described, no scientific value whatever. There is no mention of locality, season, direction of wind, swell or surface current, no mention of the relation between the effective area of the "block of wood loaded to sinking" and the barrega or breaker which floated it; the depth is spoken of as indifferently one hundred or five hundred fathoms; above all, no mention is made of any means being taken to distinguish between an apparent and real set of the breaker. It is quite clear that if the loaded block was lowered into still water, the breaker to which it was attached was, to a certain extent, moored, and the surface drift of the boat away from it would give it all the appearance of moving in the opposite direction. No mention is made of the method adopted to discriminate; or, in fact, of any method at all being adopted, or any attempt made to eliminate or neutralise the many errors which necessarily find their way into such an observation; all that we are told is that "it really appeared as if some monster of the deep had laid hold of the weight below, and was walking off with it." To such an account one is almost attempted to add—very like a whale.

I have dwelt on the thoroughly unsatisfactory nature of this experiment, because, from the description of it having been repeated in every edition of the "Physical Geography of the Sea," I find it constantly referred to—as Mr. Johnston has now referred to it—as a conclusive proof of the existence of strong counter under-currents at great depths; whereas in reality it is a proof of nothing, unless, perhaps, of the careless style of observing which was accepted as sufficient twenty years ago.

The other instance which Mr. Johnston brings forward would be really remarkable, if we only had some evidence of it as a fact; he speaks of the warm water of the Atlantic dipping down beneath the cold and "specifically lighter" water of the east Greenland current. It has been well known, long before the late German expedition, that at the meeting of the two waters there is a distinct line of demarcation, but such a line does not necessarily indicate a dip of either water, such as Mr. Johnston describes; as indeed has been very fully shown by the survey of the nearly vertical "cold wall" of the United States, along which the line of demarcation is more distinct than anywhere in the world. And besides, can we admit that the water of the East Greenland current is "specifically lighter" than that from the Atlantic? that the cold water is lighter than the hot, the salinity of the two being very nearly equal? Captain Maury speaks of hot water, like oil, running over cold; Dr. Carpenter illustrates the same idea in a long glass trough, showing plainly enough the way in which he conceives the interchange to take place. I do not attach so much value as Dr. Carpenter does to this illustration, which represents a system of motion entirely different from that of the ocean-currents; but accepting it as the exposition of the views held by the leading supporters of the claims of temperature and density, it is utterly antagonistic to the idea of this extraordinary dip of warm water said to take place near the east coast of Greenland. Whether we consider it from a purely theoretical or from a geographical point of view, the idea is wholly unsupported, and can only be classed as one of those crude speculations which, in every branch of science, do so much harm by tending to unsettle the minds of those who indeed take an interest in the subject, but have not made it a special study. J. K. LAUGHTON

Draper's Experiment simplified

WISHING to repeat Draper's Experiment, and casting about for a simple method of performing it, it occurred to me to take advantage of the intense heat evolved in the combustion of sodium, and, beginning with the entire spectrum, watch its degradation as the heat declined; to which end I formed a shallow, conical cup of thin copper wire, half an inch in diameter, and, putting therein a piece of sodium, applied a spirit-lamp till

it burst into flame ; very soon the mass melted and rose to an intense, white heat, the air streaming in through the spiral greatly favouring the combustion, a full spectrum of the utmost purity and splendour was formed, which continued as long as the white heat lasted, but afterwards declined from, and rapidly at, the violet end through the whole spectrum to the red, which persisted longest. On repeating the experiments, and raising a very narrow slit to the spectroscope, I found, as I anticipated, the sodium line reversed, and I had before me a miniature sun, a glowing centre emitting light of every wave-length, while the melted sodium, flowing through the intervals between the wires of the lower part of the cone and being dissipated by the heat, surrounded the liquid centre with an atmosphere absorbing light of its own refrangibility.

Thinking some of your readers would like to repeat so simple and inexpensive an experiment, I have ventured to send you this. T. F.

St. Mary Church, Torquay

A Wind-direction Rain-gauge

In your issue for yesterday, March 30, at page 433, you give a paragraph respecting a "Wind-direction Rain-gauge." Might I be allowed to observe that a gauge on the same principle has been in use at this place for many years, "it is arranged for *four vessels*" only, showing N. to E., E. to S., S. to W., and W. to N. Wm. Lyall

Literary and Philosophical Society, Newcastle-upon-Tyne

Entomological Queries

Can any of your entomological readers refer me to any works or memoirs on British Ants published since the appearance of Westwood's "Classification of Insects," and not quoted by Mr. Smith in his volume on "Formicidæ" in the British Museum Catalogue? What is the scientific name of the Texas agricultural ant and of the smaller ant which it ejects from its colonies? And where can I find M. Lespes' paper or papers on the "Domestic Economy of Formicaries," especially with regard to the Clavigers and other blind beetles? A. Emmet

Feb. 21

Rain produced by Fires

In your No. of Feb. 16, there is a letter from Mr. Laughton on the Artificial Production of Rains, which is worthy of notice from a strictly scientific point of view. I have little doubt that rains have been in comparatively rare cases caused by large fires. We may dismiss from our minds the idea that rains can be produced, even when the conditions are favourable, by all the powder that is burnt during a battle on land or sea. It is said that "in a problem of this nature, negative examples have more weight than positive." But it is surely more philosophical to hold that the one class of instances is as valuable as the other. If rains have sometimes been produced by fires, it is as well to try to eliminate the conditions under which they occur as in those cases in which they do not occur.

It is curious enough that much of the popular belief as well as a disbelief in the connection between rains and fires must be ascribed to the late Prof. Espy. After laying down with scientific accuracy the atmospheric conditions for such an occurrence, he somewhat lost sight of the principles in his instructions to the farmers of the United States for burning their felled timber for the production of rain. I need not enter into these. The negative cases are found to be numerous enough. Great fires rage over the Prairies and through the woods in America for weeks during the autumn, and the air becomes darkened by a veil of smoke, while no clouds are to be seen. This usually occurs when the wind is from the west and the air dry, and naturally blue and bright till obscured by smoke. In such instances the theory of fires producing rains justly enough becomes unpopular.

On the other hand Espy laid down with great precision the conditions under which rains will result from great fires in "a high dew-point and a calm atmosphere." In short, the air must be pretty well saturated with moisture, and verging upon that unstable state of equilibrium under which cumulus clouds are formed. As Espy has shown, and every observer of the phenomena will confirm, the gorgeous cumulus clouds of summer are not seen when the air is much disturbed by winds. Their very formation and existence depend upon ascending currents. Previous to Espy's investigations, it was supposed that the formation of cumulus was due to the expansion of the vapour of water by the heat of the sun and its consequent diffusion upwards through the permanent gases till it was condensed above. He clearly showed by experiment that vapour has little power of permeating air under the ordinary pressure of the atmosphere. And he drew the inference that it could be only carried into the higher strata by ascending currents. This, I think, is amply borne out in the formation of all clouds.

Fires then, are only likely to produce rains during comparatively calm weather. In the positive instances given by Espy, most of the observers state that the air was calm and sultry. One of the staff of the United States surveyors when in Florida mentions, that by firing the bush "whenever there was no wind stirring, we were sure to get a shower." Great fires are comparatively rare in this country, and I have never seen the formation even of cloud here from such a cause. However, I did once observe this phenomenon. Many years ago when sailing up the Mississippi near its mouth, in a clear and cool evening, after the subsiding of a "Norther," a great fire was burning among the reeds on its west bank. Above the dark smoke the true cumulus cloud was distinctly formed. Its bright and rounded form was beautifully brought out in the setting sun. No other clouds were visible around, and these were soon left behind as we ascended the river.

If we reflect on the matter it is difficult to conceive how clouds could be formed by means of fires during windy weather. The ascending columns could not be formed under such conditions, for the heated air would be rapidly swept off, and diluted with the mass of air rushing past. This may be illustrated by other examples better known than the formation of the cumulus. Volcanoes are well known to produce at times clouds as well as rains. But all the vapour or heat that Vesuvius could emit during an active eruption would not produce rains when a strong and dry north-west wind was blowing across its top. So also the beautiful and true cumulus cloud that so often hovers over the Falls of Niagara is only seen in calm weather. Under favourable atmospheric conditions I have lately been informed by Dr. Henry Washington that the Niagara cloud sometimes gives rise to rains and electrical phenomena. The true inference seems to be that great fires will not produce rain, excepting "the air is calm, and the dew-point high." Robert Russell

Pilmuir, Leven, Fifeshire,

A BILL TO ESTABLISH THE METRIC SYSTEM OF WEIGHTS AND MEASURES

The following Bill, prepared and brought in by Mr. J. B. Smith, Sir Charles Adderley, Sir Thomas Bazley, Mr. Graves, Mr. Baines, Mr. Albert Pell, Mr. Muntz, and Mr. Dalglish, has been ordered to be printed by the House of Commons:—

Whereas it is desirable that the weights and measures of the United Kingdom should be decimalised, and made to correspond with those of other countries.

And whereas the use of metric weights and measures is now legal, but no provision has been made for procuring the standards of said metric weights and measures, and for verifying and stamping those in use under the said Act.

Be it enacted by the Queen's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons in this present Parliament assembled, and by the authority of the same as follows:—

1. From and after the expiration of ( ) years after the passing of this Act, the length of the metre to be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standard in Paris, having the words and figures "Standard Metre, 1871," engraved upon it, and kept in the custody of the Warden of the Standards, shall be and is hereby declared to be the unit or only standard measure of lineal extension, wherefrom or whereby all other measures of extension whatsoever, whether the same be lineal, superficial or of capacity, shall be derived, computed, and ascertained, and all such measures shall be taken in decimal multiples or decimal parts of their respective units.

2. The unit of the measure of surface shall be the square of ten metres, which shall be and is hereby denominated the "are."

3. The unit of the measure of capacity, as well for liquids as for dry goods, shall be the cube of a tenth of the metre, and the same shall be and is hereby denominated the "litre."

4. The unit of weight shall be and is hereby denominated the "gram." A thousand grams shall be and is hereby denominated the "kilogram." A standard of the kilogram shall be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standards in Paris, and have the words "Standard Kilogram, 1871," engraved upon it, and the same shall be kept in the custody of the Warden of the Standards.

5. For the more convenient use of metric weights and measures, it shall be lawful to take the double and the half of all the said units, and their decimal multiples and decimal parts.

6. The said weights and measures hereby established shall be and are hereby denominated the standard metric weights and measures, as shown in the table hereto annexed.

7. Copies and models of the same standard metre and kilogram shall be sent to the Lord Mayors of London and Dublin, to the Lord Provost of Edinburgh, and to all counties, shires, stewartries, ridings, divisions, cities, towns, liberties and places in which by law copies and models of the standard imperial weights and measures are required to be kept, and to such other places and persons as the President of the Committee of the Privy Council for Trade may from time to time direct.

8. All judges, magistrates and other person or persons who now are or shall hereafter be authorised by law to order or provide copies of the present imperial standard weights and measures, shall at all times hereafter have like power and authority in every respect to order and provide copies of the standard metric weights and measures, and to charge the expenses thereof upon the fund or funds, money or moneys, that would have been liable in case it had been copies of imperial weights and measures that had been ordered or provided.

9. All and every the provisions and provision which are by law in force with respect to the inspection, verification, reverification, stamping, counterfeiting and modes of conviction, with the penalty or penalties relating thereto, of the present imperial standard weights and measures, shall apply to and be in force with regard to the standard metric weights and measures in every respect as if the said standard metric weights and measures were comprised in and designated by the imperial weights and measures in the Acts relating to such inspection, verification, reverification, stamping, counterfeiting and modes of conviction, and the penalty or penalties relating thereto as aforesaid.

10. From and after the expiration of ( ) years from the passing of this Act, the imperial and all local or customary weights and measures shall be abolished, and every person who shall sell by any denomination of weights or measures other than those of the standard metric weights and measures, or such decimal multiples or decimal parts thereof as are authorised by this Act, shall, on conviction, be liable to a penalty not exceeding the sum of 40*s.* for every such sale.

11. From and after the expiration of ( ) years after the passing of this Act, if any person or persons shall print, or if the clerk of any market or other person shall make any return, price list, price current, or any journal or other paper containing price list or price current in which the denomination of weights and measures quoted or referred to shall denote or imply a greater or less weight or measure than is denoted or implied by the same denomination of the metric weights and measures under and according to the provisions of this Act, such person or persons or clerk of the market shall forfeit and pay any sum not exceeding 10*s.* for every copy of every such return, price list, price current, journal, or other paper which he or they shall publish.

12. As soon as conveniently may be after the passing of this Act, accurate tables shall be prepared and published, under the authority of the Committee of Privy Council for Trade, showing the proportions between the imperial weights and measures heretofore in use and the standard metric weights and measures hereby established, with such other conversions of weights and measures as the said Committee of the Privy Council for Trade may deem necessary, and after the publication of such tables all future payments to be made shall be regulated according to such tables.

13. And whereas the weights and measures by which the rates and duties of the customs and excise and other Her Majesty's revenue have been heretofore collected, are different from the metric weights and measures directed by this Act to be used: It is hereby enacted, that so soon as conveniently may be after the passing of this Act, accurate tables shall be prepared and published under the direction of the said Committee of the Privy Council for Trade, in order that the several rates and duties of customs and excise, and other Her Majesty's revenue, may be adjusted and made payable according to the respective quantities of the standard metric weights and measures directed by this Act to be used, and that on the expiration of ( ) years after the passing of this Act, the several rates and duties thereafter to be collected by any of the officers of Her Majesty's customs or excise, or other Her Majesty's revenues, shall be collected and taken according to the calculations in the tables to be prepared as aforesaid.

14. From and after the passing of this Act, and until the use of the metric weights and measures shall be made compulsory, the said metric weights and measures shall be deemed and taken to be legal weights and measures, and as such may be used for all purposes whatsoever.

15. As soon as conveniently may be after the passing of this Act, the metric standards to be provided under this Act shall be placed in the custody of the Warden of the Standards, and the Committee of the Privy Council for Trade shall cause the metric weights and measures in use under the present Act to be verified and stamped in the same manner as the imperial weights and measures are now required to be.

16. From and after the passing of this Act the "Metric Weights and Measures Act, 1864," shall be and is hereby repealed.

FLOATING ISLANDS IN VICTORIA

GIPPSLAND is a province of Victoria. It is bounded by the Australian Alps on all sides except on the south, which the sea washes for over 100 miles. It may be called the Piedmont of Australia, rich fertile plains intersected by rivers flowing into a lake system extending all along the coast, and separated from the sea by a sandy narrow ridge, with one navigable opening. From a local paper, the *Gippsland Times*, I send the following description of "floating islands" on the lakes.

The alluvial deposit constantly brought down from the mountain ranges by the numerous rivers in this district, enables us to see a very decided process of land making continually going on, and thus teaches a useful lesson in geology.

AUSTRAL-ALPINE

Melbourne

"As one of the Gippsland Steam Navigation Company's steamers was recently crossing Lake Wellington, the man at the wheel suddenly observed land right in the track of the steamer, apparently only a short distance from the straits separating Lakes Wellington and Victoria. He called the captain's attention to the strange sight, and on coming up close, the land was discovered to be a small island, about thirty yards in length by twenty broad. It was covered with a rich coating of luxuriant grass; and small trees, tea tree, and bush shrubs appeared to be growing in profusion. The only occupants of this remarkable apparition were a few pigs, feeding away contentedly and apparently enjoying their novel journey by water. A second island of the same description, but much smaller, was noticed a little farther on, but this had evidently detached itself from the larger piece of land, or most probably had been separated by the rooting depredations of the porkers. From what portion of the main land this floating island came, is, of course, matter of conjecture, but it is known that a portion of the soil at Marley Point, on the southern shore of Lake Wellington, became detached recently, and floated miles across the lake with some twenty or thirty head of pigs aboard. As long as the wind drove it in that direction, the island drifted towards M'Lennan's Straits, but a change of wind brought it back again, after a three days' trip, within a mile of the spot from which it had broken away. We believe it is the opinion of the district surveyor, Mr. Dawson, that the area of the Roseneath run, west of Lake Wellington, has been increased some twenty or thirty acres by the addition of drift islands."

WILHELM von HAIDINGER

WILHELM VON HAIDINGER is no more. He died after some years of failing health, though the illness to which he finally succumbed on the 19th of March was a short one. Among his veteran contemporaries in the mineralogist's craft, such as Breithaupt, Karl F. Naumann, Gustav Rose, and Karl C. von Leonhard, he must have stood second on the ladder of time, the venerable Breithaupt being some four years his senior. His father, Karl Haidinger, was a mineralogist, and indeed was for several years Professor of Mining at Schemnitz. But, while Wilhelm was yet an infant, his father died at Vienna, where he had, in his latter days, filled a post in the Imperial Mint.

The young Haidinger seems in some sort to have inherited his father's taste for minerals, for he joined the class of Mohs at Gratz, where that distinguished mineralogist was giving a new impetus to the study of his science by popularising it in what was termed a natural history system of classification, and by a systematic method of discriminating the different species of minerals; and subsequently young Haidinger went to Freiberg to complete his training in Mining. Count Breunner, who came to England in 1822, and was made a Doctor in Civil Law at Oxford, invited the young mineralogist to accompany him. He embraced the offer, and they travelled together through England, and together reached Edinburgh, where the energetic and winning character of the young Austrian, fresh with the lore of the famous lecture-room at Gratz, at once made him friends in the Northern Athens, in the University of which capital Jameson had already made Minerals a fascinating study. Among the friends he there made was Mr. Allan, the wealthy banker, who during the next year invited young Haidinger to make a home of his house while employed in translating the Mineralogy of Mohs into English. So after returning to Vienna, he once more, in 1823, came to Edinburgh, and made Mr. Allan's house his head-quarters till 1827. He appears to have been a sort of tutor to Mr. Robert Allan, the eldest son of his generous friend; and with him he travelled during these four years through Cornwall, and then through Norway, Sweden, Denmark, Germany, Austria, Italy, and France. It was mainly during these travels that the famous collection, afterwards the property of Mr. Robert Greg, and now in the British Museum, was formed.

During these four years he brought out his translation of Mohs' treatise, and wrote several Mineralogical papers for the Wernerian Society and the Transactions of the Royal Society of Edinburgh. Subsequently he joined with his brothers in starting a porcelain manufactory at Elbogen near Carlsbad. Here he continued till 1840, still, however, bringing out from time to time memoirs on new minerals or new observations on minerals already known. The minerals Edingtonite, Sternbergite, Fergusonite, Herderite, Erinite, Picrosmine, Johannite, Botryogen, and Hartite, are among those he studied and described previous to and during this period.

In 1840 he returned to his native city, Vienna, to devote himself more exclusively to the scientific pursuits he loved. Thenceforward his memoirs will be found distributed at pretty regular intervals through the Sitzungsberichte of the Vienna Academy.

Among the subjects that he worked at during the next period of his life were the optical phenomena exhibited by crystals in regard to light and colour; more particularly those of pleiochroism. He invented, for the investigation of these, the Dichroiscope, a simple but useful little instrument, enabling an observer to examine and compare the different characters of the absorption exercised by a birefringent crystal on light traversing it, according as the plane of vibration of the light is parallel or perpendicular to any one of the principal sections of the crystal. The description of Hauerite, a new mineral, in fact, a manganese pyrites, was given in 1847; that of Kenngottite in 1857. The Haidinger brushes, a subjective phenomenon due to the eye itself, and observed in looking towards a window through a tourmaline or Nicol prism, was an illustration of the acuteness of his powers of observation. A compendious and valuable treatise on Mineralogy, brought out in 1845, to take the place of an earlier treatise, was also, during this period of his life, continually undergoing revision for new editions; while new investigations of minerals were also appearing under his name

From the moment of the foundation of the Geological Institute for the Empire in Vienna, Haidinger was the obvious man to lead that younger generation by whose labours the new Institute was to be reared and supported. So he was its Director until some two or three years ago, when he retired from the position he had filled so well, with a Ritter's rank and a well-earned pension.

For the last twelve years of his life he had given his attention, almost to the exclusion of other scientific inquiry, to the subject of meteorites. He laboured indefatigably almost to the last in collecting specimens from any new falls of meteorites reported in any portion of the globe, that they might be added to the noble collection in the Imperial Museum; and he was always at work at the interpretation of the strange phenomena witnessed by those who have described the fall of meteorites in any language or country.

Such is a rapid review of the main features in the life of a man who seems always to have been at work; whose pen was one of the readiest and busiest; whose nature was ever genial and generous; and who, at the age of seventy-seven, has finished an honourable life's work, and leaves behind him a name which Austria may cherish as that of one of her illustrious sons, and which many an Austrian and many a foreigner will remember with warm respect; while those who enjoyed nearer relations with Wilhelm von Haidinger will assuredly ever remember him with affectionate regard.

N. S. M.

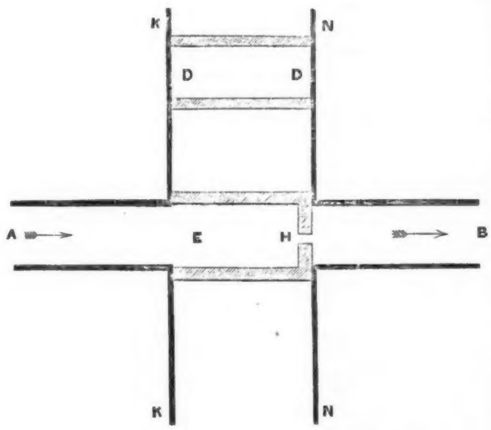
A TUBULAR POSTAL SERVICE

SOME large iron pipes have just been laid from the General Post Office to the Branch Office at Charing Cross, through which pipes packages of letters are blown in either direction at will, by compressed air. These tubes are to be extended from Charing Cross to the Houses of Parliament; and when the total expenses of laying pipes and of transmitting small packages through them is known by experience, very possibly the system may be extended, and letters intended for quick delivery may be sent by this method at a moderate charge.

This plan of sending messages through pipes for short distances has been employed in the City for many years in connection with the late Electric and International Telegraph Company. Seventeen or eighteen years ago, Mr. Latimer Clark laid down tubes from the Central Office of the Company in Lothbury to the Telegraph offices in Cornhill and Mincing Lane. By means of a steam-engine which worked a great air-pump, messages enclosed in small gutta-percha carriers, each somewhat resembling a sausage in shape and size, were drawn from Cornhill and Mincing Lane to Lothbury. Additional and smaller pipes were afterwards laid down by him, so that the vacuum could be applied to the further ends of the carrying pipes, in order that messages might be sent in the opposite direction also. They were then easily transmitted to and from Cornhill, but the Mincing Lane station being two-thirds of a mile off, it was found that the friction of the air in the pipes was too great, so that carriers could be sent in one direction only, namely, from Mincing Lane to Lothbury. Some years later, when Mr. C. F. Varley became engineer to the International Telegraph

Company, he employed compressed air to drive carriers to out-stations, and a vacuum to bring them back again. When a vacuum is employed, the carriers are driven by the ordinary atmospheric pressure only of fifteen pounds to the square inch, but when condensed air is employed, almost any pressure may be applied, so that the carriers can be driven with enormous velocity. He also substituted felt for gutta-percha carriers, since the latter were sometimes partially melted by the heat occasioned by friction, and coated the insides of the pipes with sticky matter. Further, he designed some pneumatic valves; the carriers, on arriving at the end of their journey, were made to strike against a brass button, the motion of the button set a valve to work, the valve opened the door of the chamber at the end of the pipe, the carrier then fell out, and dropped down on the table below. Thus the carriers were made to let themselves out when they arrived at the end of their journey, by which plan much hand labour on the part of assistants was saved. These improvements worked well, and are working well at the present time. Seven or eight City telegraph stations have been thus pneumatically connected for many years.

But a further improvement in the system has been made within the past year or two by Mr. C. W. Siemens. He lays down the pipes in circuits, and has pressure in the rear, and a vacuum in front of each carrier, so that as the



motive forces all act in one direction, there may be several carriers flying through the tubes at the same time. If these carriers were not stopped anywhere, they would all find their way back to the central station. He has invented also a "shunt," whereby any intermediate station can stop its own carrier, and pick it out of the tube without interfering with the motion of other carriers which may be flying through other parts of the circuit. Suppose the carriers to be three minutes apart in point of time, and that five stations are on the circuit, each station knows at what time its own carrier is due, so is able to take it out without interfering with other carriers. If the carriers be not sent at regular intervals of time, the receiving station can be told when its carrier is started, by telegraph. The plan of picking out the carriers is simple, and the principle may be explained by the aid of the accompanying diagram. A B is the main tube, and the direction taken by the carriers is denoted by the arrows; K K and N N are metal plates, between which the two short tubes D D and E H slide in an air-tight manner. These two short tubes are attached to a lever joint. When the assistant does not wish to intercept a carrier, the tube D D is left in the place occupied by the tube E H in the cut, and D D being open at both ends, carriers pass through it without interception. Upon sliding E H into the gap in the main tube, however, the carrier is stopped by it. It is brought to a standstill very gently, because it compresses some air in front of itself, which air issues with restricted freedom through the hole H; thus the carrier makes for itself an air-cushion to break the violence of the blow. When the carrier is caught, the tube D D is brought into the line of the main tube, after which an air-tight door in the side of E H is opened, and the captured carrier, with its messages, is taken out.

For short distances to and from telegraph stations near the Bank and the Post Office, the pneumatic tubes are from one-and-a-half to two-and-a-half inches in diameter; they vary in diameter according to distance. But the one large Siemen's circuit at present laid in London goes from Telegraph Street to Charing Cross and back, the General Post Office and the Temple Bar Office being the intermediate stations; this pipe is three inches in internal diameter. The carriers travel at the rate of about a mile in three minutes, but the rate varies with the pressure.

It may be asked why these pneumatic tubes are useful in connection with telegraphic offices? The fact is, that there are many disadvantages in sending messages very short distances by the electric telegraph. Suppose one telegraphic wire be suspended between two stations half a mile apart, and another be suspended between two stations three hundred miles apart; let thirty messages be received all at once for transmission over each of these two wires, it is plain that some of these messages will have to wait half an hour before their turn comes to be signalled over the wire. The public will not complain of a delay of half an hour in the delivery of a message in a town three hundred miles off, but they would make a great outcry if a message took half an hour to go half a mile by the electric telegraph. Therefore, it is the simplest and most expeditious plan for the central telegraph station in a great city to blow the messages bodily through tubes, to branch stations not far off; the plan saves time and saves labour. Complaints published in the newspapers about delays in telegraphic messages, refer for the most part to telegrams sent from one part of London to another, and the delays are often caused by the pressure of a sudden influx of work upon particular wires. WILLIAM H. HARRISON

NOTES

WE learn that the volume containing the various observations of the recent total eclipse will be edited by the Astronomer Royal.

IT is stated that Mr. Abel, Prof. Ramsay, and Mr. Huggins, have been invited to lecture this year to the members of the British Association at the forthcoming meeting at Edinburgh.

THE meeting of the Royal Colonial Institute on Monday is likely to have a practical result. The paper read was by Mr. Hyde Clark on the "Appointment of a Reporter on Trade Products for the Colonial Office." After an interesting discussion the President, Lord Bury, M.P., on the part of the Council, proposed that a Committee should be named to apply to the Secretary of State for the Colonies for such a department, and for the provision of a Colonial Museum on the same I a is as the department provided at the India Office for India.

AT the last anniversary meeting of the Chemical Society it numbered 551 ordinary members and 36 foreign members. Six of the former have withdrawn from the Society,—on the other hand forty-two new members have been elected into the Society. It has lost five ordinary members by death, viz., Mr. George Jolley, Dr. W. A. Miller, Dr. Aug. Matthiessen, Dr. J. S. Muspratt, and Mr. W. W. Rouch; and the deaths must also be recorded of two foreign members, viz., Prof. Gustav Magnus and Prof. Weltzien. The election of the president, the officers, and the other members of Council for the ensuing year

was then proceeded with, and the following is the list of the gentlemen elected:—President: Frankland, E., D.C.L., F.R.S. Vice-Presidents, who have filled the office of President: Brodie, Sir B. C., F.R.S.; De la Rue, Warren, Ph.D., F.R.S.; Hofmann, A. W., D.C.L., F.R.S.; Playfair, Lyon, Ph.D., C.B., F.R.S.; Williamson, A. W., Ph.D., F.R.S.; Yorke, Col. P., F.R.S. Vice-Presidents: Debus, H., Ph.D., F.R.S.; ilbert, J. H., Ph.D., F.R.S.; Noad, H. M., Ph.D., F.R.S.; Odling, W., M.B., F.R.S.; Redwood, T., Ph.D.; Stenhouse, J., Ph.D., F.R.S. Secretaries: Harcourt, A. Vernon, M.A., F.R.S.; Perkin, W. H., F.R.S. Foreign Secretary: Müller, H., Ph.D., F.R.S. Treasurer: Abel, F.A., F.R.S. Atkinson, E., Ph.D.; Bassett, H.; Bloxam, C. L.; Dupré, A., Ph.D.; Field, F., F.R.S.; Holzmann, M., Ph.D.; M'Leod, H.; Mills, E. J., D.Sc.; Roscoe, H. E., Ph.D., F.R.S.; Russell, W. J., Ph.D.; Smith, R. Angus, Ph.D., F.R.S.; Voelcker A., Ph.D., F.R.S.

DR. GEORGE BURROWS, F.R.S., Physician Extraordinary to the Queen, has been elected President of the Royal College of Physicians, in succession to Sir James Alderson.

A THIRD (revised) edition is now in the press of Mr. Darwin's "Descent of Man."

MR. JAMES CROLL calls our attention (*à propos* of our notice of his paper "On the Cause of the Motion of Glaciers," No. 68, p. 309) to the fact that he does not conclude the age of the sedimentary rocks to be 1,036,800,000 years, but *assumes* the period—for reasons stated on a former occasion—to be only 100,000,000 years. The drift of the paper was to point out a "new method" of determining the (mean) thicknes of the sedimentary rocks. The method leads to the conclusion that their thickness cannot be much over 2,500 feet!

DR. HOOKER has just started on a botanical expedition of eight or ten weeks into the interior of Morocco, a hitherto almost untried field. He is accompanied by Mr. R. Ball and one of the gardeners from Kew to assist in collecting plants.

WE learn from the *Journal of Botany* that Dr. Karl Heinrich Schultz-Schultzenstein, of Berlin, one of the most eminent botanists in Germany, was found dead in his bed on the morning of March 23rd. He had been engaged at his desk till past midnight. The deceased, though in his seventy-third year, was remarkably active, and was lecturer on physiology, as well as on botany, in the University of Berlin, with which he had been connected since 1822.

THE Medical Scholarship for Women in Edinburgh University of the value of 50*l.* for three years, offered for competition by Mrs. Garrett Anderson, M.D., and two other ladies, has been gained by Miss Annie Barker, daughter of Dr. Barker of Alderhot. It was awarded according to the results of the preliminary examination in Arts in the University.

WE learn from Paris that M. W. De Fonvielle was sentenced to death by the insurgents in consequence of an article published in the *Times* of March 27. We have however had the satisfaction of receiving from him this week our usual budget of Paris news. The only men of science who have ranked with the insurgents are M. Le Français, a former teacher in an elementary public school, M. Jules Allix, inventor of a new system of orthography, M. Charles Emmanuel, who opposes the theory of the rotation of the earth from west to east; Dr. Robault, a homœopathic practitioner, and M. Leroy, a foreman employed by Messrs. Hachette and Co. for reading for the press. A false rumour was circulated in the Quartier Latin that they were to send a delegate to take possession of the Observatory, the Ecole de Médecine, the Ecole de Droit, College de France, Institute, and Jardin des Plantes. All these establishments have, however, been left undisturbed in the hands of the scientific authorities.

LAST week the *Journal Officiel* of the insurgents printed the account of the sitting of the Institute. But "reactionary" papers having sharply commented on the piracy, the *Officiel* abstained from mentioning the sittings of that assembly. The students have to a man ranked amongst the defenders of order. Almost every educational establishment is closed during "the revenge of science" proclaimed by the Commune. Pupils of the Polytechnic School were sent home, lectures at the Sorbonne, College de France, and Conservatoire des Arts have been stopped. Libraries are closed, and no books are being published or selling in Paris. There is no question of reform as long as the rebels enjoy their rule, and the Garde Nationale their thirty sous a day.

THE Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons concluded his course on the Characters and Modifications of the Teeth of Mammalia on Wednesday, the 29th ult. In the last lecture the methods of drawing inferences as to the affinities and habits of extinct animals from dental characters were illustrated by the much controverted case of *Thylacoleo carnifex* of Owen, an extinct Australian marsupial, known at present only by its skull and teeth. The animal is supposed by its original describer to have been one of the fellest and most destructive of predatory beasts, and to exemplify the simplest and most effective dental machinery for predatory life and carnivorous diet known in the Mammalian class, a proposition which Prof. Flower contested, showing by comparison with all the recent marsupials, that its affinities are with the existing diprotodont species (kangaroos, potaroos, and phalangers), none of which are purely predaceous and carnivorous, and that, therefore, there can be no reason for inferring that *Thylacoleo* had such habits, unless any special modifications of its teeth towards the carnivorous type could be indicated. This was, however, shown not to be the case, by comparison with all the various known truly predaceous carnivores, whether belonging to the placental or to the marsupial type of mammals. There are, therefore, no grounds for the assumption on which the name of the animal is based. But, on the other hand, neither can it, according to Prof. Flower, be classed among herbivores in the ordinary sense of the word; and all arguments against its "herbivority" derived from the structure of its molar teeth have no bearing on the proof that it was lion-like in its habits, as there are numerous alternative suppositions. Indeed, the teeth of this remarkable animal are so highly specialised and unlike those of any actually existing species, that it is impossible from analogy with recent forms to deduce its mode of life with any certainty, its organisation having in all probability been in conformity with some surrounding conditions which have now passed away.

AT a meeting of the American Ethnological Society of New York, held in October 1869, a committee was appointed for the purpose of organising a new body upon the basis of the society just mentioned, to be entitled the Anthropological Institute of New York. This committee lately issued invitations to the members of the Ethnological Society, and others interested, to attend at the house of Mr. E. G. Squier on the 19th of March, in order to complete the proposed arrangements by adopting a constitution and by-laws, to be formally presented to the meeting. In the present interest which attaches to studies relating to the past and present history of mankind and the development of civilisation, it is much to be hoped that this new society will establish itself on a firm foundation in New York, and carry out the mission projected for it by its founders.

FOR some years Dr. Burmeister, an eminent German naturalist and physicist, from Halle, has resided in Buenos Ayres, in charge of the National Museum in that city, and by his nvestigations and publications concerning specimens belonging to the Museum has given to it a great reputation. Quite recently, as we learn from *Harper's Weekly*, a murderous

attack was made upon him by one of his servants, which was happily frustrated; but the newspaper comments upon the transaction developed the existence of so much animosity or jealousy toward foreigners on the part of the people that the doctor has finally determined to resign his position and return to Germany. Dr. Sarmiento, the President of the Republic, it is said, has endeavoured to change this determination, but apparently without effect. In parting with Dr. Burmeister, Buenos Ayres will lose one who has given to the country that position in science through his writings that Dr. Sarmiento has in literature, and his loss will not easily be made good. It is an interesting fact that both the National Museums of Chili and of the Argentine Republic are presided over by German naturalists, the director of the latter being Dr. R. L. Phillippi, well known in the scientific community.

IN June next Professor Birkett will commence his course of lectures on the Nature and Treatment of New Growths, at the Royal College of Surgeons, on the conclusion of which Mr. Hulke will deliver three lectures on the Minute Anatomy of the Eye.

WE learn from the *British Medical Journal* that Dr. E. Klein of Vienna has been appointed Assistant Professor in the new laboratory in connection with the Brown Trust for Experimental Pathology, which is about to be erected in London. Dr. Klein has been Professor Stricker's assistant for several years, and has contributed much to that author's "Handbook of Histology," now being published in English by the New Sydenham Society. By this change, Austria will lose and this country gain one of the most promising of young histologists. As an investigator and teacher of the structure of tissues, Dr. Klein has been for several years held in much esteem in Vienna.

THE following are the arrangements for the Lectures at the Royal Institution of Great Britain after Easter, 1871:—On Tuesdays, April 18 and 25, and May 2, William Pengelly, F.R.S., will deliver three lectures "On the Geology of Devonshire, especially of the New Red Sandstone." On May 9 and 16, Charles Brooke, F.R.S., will lecture "On Force and Energy." On May 23 and 30, and June 6, the Rev. Professor Haughton, M.D., F.R.S., will lecture "On the Principle of Least Action in Nature, Illustrated by Animal Mechanics." On Thursdays, April 20 to June 8, Professor Tyndall, LL.D., F.R.S., wil deliver eight lectures "On Sound;" and on Saturdays, April 22 to June 10, J. N. Lockyer, F.R.S., will deliver eight lectures "On Astronomy." The lecture hour is three o'clock. The following are the probable arrangements for the Friday evenings after Easter, 1871, to which members and their friends only are admitted:—April 21, Professor Blackie, F.R.S.E., "On the Pre-Socratic Philosophy." April 28, Professor Odling, F.R.S. May 5, W. R. S. Ralston, M.A., Trinity College, Cambridge, "On Russian Folk-Lore." May 12, Professor Huxley, F.R.S. May 19, Colonel Jervois, R.E., C.B., Secretary of the Defence Committee, and Deputy Director of Fortifications, "On the Defence of the United Kingdom." May 26, Sir J. Lubbock, Bart, M.P., F.R.S., "On Relationships." June 2, Professor Thomas Andrews, F.R.S., Principal of Queen's College, Belfast, "On the Gaseous and Liquid States of Matter." June 9, Professor Tyndall, LL.D., F.R.S.

PROF. WINCHELL, director of the Geological Survey of Michigan, has lately presented a report of the progress of the survey from its inauguration, May 1869, to November 1870. He sketches an outline of the nature and extent of the researches he proposes in connection with the investigations, and expresses a desire for sufficient appropriations to enable him to complete his work in the shortest possible time, two years being suggested as sufficient with proper means. His plan includes, in addition to pure geology and mineralogy, such subjects as palæontology, climatology, natural history, ethnology, &c. The sum of 61,300 dols. is asked for by the Professor for the purpose of completing his field work, as well as of preparing the necessary maps and illustrations for his report.

WE have information of the departure of M. Miclucho Maclay, of Russia, in the Russian steamer *Witiaz*, for a seven or eight years' cruise in the Pacific—the first two of which are to be expended in the investigation of the island of New Guinea. This region, as is well known, abounds in objects of natural history of the greatest interest, although comparatively little, so far, is known of its features in detail.

THE Royal Belgian Academy of Science, Letters, and the Fine Arts has just issued its thirty-seventh *Annuaire*, containing a historical sketch of the Academy, and biographical sketches of the following members who died during the year:—François Joseph Navez, painter, with a list of his works and pupils; Edward Ducpetiaux, political economist and prison reformer; Charles-Fréd.-Phil. von Martius, the celebrated botanist; Edouard Gerhard, philologist and archæologist; Prudens Van Duyse, poet; and Charles Aug. de Bériot, composer.

THE new explosive dualin has been used in the blastings required for the great Hoosac tunnel in the United States. Over 1,000lb. have been exploded since December 1, and it appears to possess the full strength of nitro-glycerin, besides being perfectly safe from any ordinary blaster to handle. It will not explode by concussion, and can be tamped as hard as powder with perfect safety. There seems no reason why it will not eventually entirely supersede common powder for all blasting purposes.

THE *Scientific American* announces a substitute for lime in the lime-light of the oxyhydrogen jet. It appears that a prism cut out of the mineral dolomite will emit a light as powerful if not superior to the calcium light. The dolomite is made up of nearly equal parts of the carbonate of lime and magnesia, and the combination of these two earths produces effects superior to what can be obtained from either of them alone. The light is said to be suited for photographic purposes, especially for copying pictures. As dolomite is an abundant rock, its application for purposes of light may prove of peculiar value.

THE report of the Manchester Field Naturalists' Society for 1870 has, according to the Secretary, "little to say except that the year's proceedings have been marked by smoothness and success, with no particular incidents to give it distinction above preceding years, and certainly without any of an infelicitous kind." The meetings have been well attended; there has been a considerable entry of new members; and the Treasurer's report is satisfactory. The report is taken up with brief *résumés* of the proceedings at each meeting, and a summary is given of a useful paper by Mr. R. B. Smart "On the Variation of Species" in the vegetable kingdom. We much regret that, from a society numbering its members by hundreds, we cannot obtain a proportionate amount of work; indeed many of the smaller bodies put the Field clubs of our large towns to shame in this respect. Mr. Grindon's "Flora of Manchester" is not only out of date, but also out of print; and a complete fauna and flora of the district would be both useful to naturalists and creditable to the society. Surely among so many members some may be found both able and willing to undertake such a work. We observe that the Secretary, in the present report, speaks of plants by their English names, some of which are of his own invention. As their scientific equivalents are omitted we are left in the dark as to some of them; "dimplewort" is, we believe, *Cotyledon umbilicus*, but "blushwort" baffles our ingenuity. The President for 1871 is Mr. Thomas Turner, F.L.S., and Mr. Grindon continues to act as Secretary,

THE time for the trial of machines for separating the fibres of the Rhea plant which are to be sent in by competition for the Indian Government prize of 5,000*l.*, has been postponed till April 1872. It is requested that notice of intention to compete be given before May of this year. Arrangements have been made for supplying some of the plant to intending competitors.

THE FIRST GERMAN NORTH POLE EXPEDITION

A NUMBER of Petermann's "Mittheilungen" published in January 1871, consists of an account of the first German North Pole Expedition by Captain Koldewy and Dr. A. Petermann. The vessel in which this expedition was undertaken was the *Germania*, a cutter of only eighty tons burden. Twelve persons sailed in her, Captain Koldewey, the commander of the party and joint author of the present memoir, R. Hildebrant, chief mate, and ten sailors. They started from Bergen May 24, 1868, the Swedish expedition sailing about the same time in a steamer. The voyage extended over four months. Dr. Petermann considers that the only practicable routes to the North Pole are either through Behring's Straits or the sea between Greenland and Spitzbergen. The latter was attempted by the Expedition.

The year turned out to be a most unfavourable one, the sea being more than usually obstructed with ice. After vainly attempting to reach the east coast of Greenland, the *Germania* crossed over to Spitzbergen, but was stopped by pack-ice. Greenland was again visited with a like result; but on a second trial of the Spitzbergen route a fortunate break in the ice occurred, and on September 14, lat. 81°·5 was reached, this being the highest point ever yet attained by a ship, although with sledges 82°·45 was reached by Parry in 1827. The east coast of Spitzbergen was visited by means of the straits (Hinlopen Strasse) which separate the smaller northern portion of this group of islands from the larger southern portion. Here a new island was discovered, and the surrounding coast-line mapped. Dr. Petermann names the island William Island, and the straits which separate it from the mainland Bismark Straits; we also find on the map Augusta Bay and Cape Moltke. Dr. Petermann rejoices greatly that the Germans have thus at last left their mark on the map. He says that it has been very trying to him to have seen for the last thirty-two years in maps of all parts of the world containing new geographical discoveries no names but "Victoria," "Wellington," "Smith," "Jones," &c. Captain Koldewey describes the glaciers of Spitzbergen as differing from those of Switzerland in the following points:—They for the most part stretch right down into the sea, where they end in a perpendicular wall. The upper surface is somewhat polished and free from all roughness and steep ice blocks. Moreover in the glaciers examined at Augusta Bay and William Island there are no crevasses. Moraines are present, those of the great glacier in Augusta Bay consisting of limestone and basalt. The actual scientific results of the expedition are very small, owing to the badness of the weather. Some fragmentary monthly isothermals of sea temperature have been constructed by Dr. Petermann from the observations made during the voyage, and are marked on the two maps which accompany the memoir. Captain Koldewey considers that the route by the East coast of Greenland is the one which should be attempted by future expeditions. The route north of Spitzbergen is impracticable, because a branch of the Gulf Stream here meets directly the cold polar current, and a barrier of ice is the result. For further exploration he advises the employment of a schooner rigged vessel of from 150 to 200 tons, with auxiliary steam power.

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine."

BY W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

(*Concluded from p.* 417.)

WE commenced our observations on the morning of Oct. 1 at the point of greatest depth (Station 65). The temperature of the surface at 6 A.M. was only 63°, which was at least eight degrees lower than the average temperature at that hour within the Mediterranean. The bottom temperature at 198 fathoms was 54°·5; and the specific gravity of the bottom water was 1028·2. The coincidence both in temperature and specific gravity with the bottom-water at Station 64 was thus very close. The place of the ship having been determined by angles taken with the shore, the rate of the surface-movement was tested as on former occasions; and was found to be 1·277 mile per hour, its direction being E. ½ S. The "current drag" was then sunk to 150 fathoms,—the greatest depth at which it was thought safe to use it; and the boat from which it was suspended moved E. ¾ N. at the rate of 0·840 mile per hour. This observation indicated a very considerable retardation in the rate of *in*-flow; but gave no evidence of an *out*-flow. It did not, however, negative the inference deducible from the temperature, and still more from the specific gravity of the water beneath, that an out-flow takes place in that lowest stratum which we could not test by the "current drag."

We then steamed across the deep channel towards the Spanish side; and passing a bank of 45 fathoms which rises near its middle, we sounded again at Station 66, about six miles to the northward of Station 65. The surface-temperature at 9 A.M. was here found to have risen to 69°; and since not more than half this increase could be attributed, according to our experience elsewhere, to the increase of direct solar radiation at this period of the day, the cause of the additional elevation has to be sought elsewhere. The length of sounding-line run out was 147 fathoms; but on attempting to reel it in, the lead was found to have fixed itself between rocks; and all Capt. Calver's skill in the management of his ship proved inadequate to free it. As we were thus anchored by our sounding-line, it was requisite to set ourselves free, by putting a breaking strain upon it; and we thus lost, with the lead, one of our water-bottles, and a pair of thermometers, one of which was specially valued by us as having been used throughout the *Porcupine* Expedition of 1869, in which the temperature soundings had proved of peculiar importance. The "current-drag" was here let down to 100 fathoms; and the boat from which it was suspended moved along in the direction of the surface-current, and at the rate of 1·280 mile per hour, which was almost precisely that of the surface-current in the previous observation.

Deeming it important to obtain the temperature and specific gravity of the bottom-water on the Spanish side of the deeper portion of the channel, we slightly shifted our ground, and again let down our lead, with thermometers and water-bottle, at Station 67, where the depth proved to be 188 fathoms. On beginning to reel in the line, we found the lead to have anchored as before, and for some time feared that we should sustain a second loss of the water-bottle and thermometers attached to it. The means taken by Captain Calver for its extrication, however, proved on this occasion successful; and we had the satisfaction of seeing the whole apparatus safely brought up,—the lead bearing evident marks of having been jammed between rocks and then violently strained. The temperature of the bottom proved to be 55°·3, that of the surface being 73°; and the specific gravity of the bottom-water was 1028·1, that of the surface being 1026·8. Here again, therefore, the evidence afforded by the temperature and specific gravity of the bottom-water was conclusive as to its Mediterranean character. Its density corresponded rather with that of the bottom-water, than with that of the intermediate stratum, at the opposite end of the Strait; but the more rapid westerly motion of the latter would seem to indicate that the water which here flows over the "ridge" is derived from it, rather than from the deeper layer, and that its diminution in density is due to the dilution it sustains in its course. In either case, the denser Mediterranean water discharged by this undercurrent must flow up-hill; but the incline is so gradual that a very small force, if constantly sustained, would suffice to produce the elevation needed to carry it over the ridge.

Whilst we were prosecuting these inquiries, our attention was attracted by the long chains of aggregate salpæ which were floating close to the ship near the surface of the very calm sea. We were able to collect four or five different species of these, and to submit them during life to microscopic examination. The reversal of the direction of the circulation took place in all at more regular intervals than we have usually found to be the case in the compound ascidians; and we were able to distinguish an unmistakable rudimentary eye, which had not, we believe, been previously noticed. We hope to be able hereafter, by the detailed study of these specimens, to make some additions to the knowledge previously acquired of this very interesting group. As the nature of the bottom put it out of the question to attempt to dredge on this ridge, our only means of investigating its zoology lay in the use of the "hempen tangles." A "sweep" taken with these brought up a few echinoderms and polyzoa of no special interest.

We now took our final leave of the Mediterranean basin with mingled feelings of disappointment and satisfaction. The zoological results of our cruise had been by no means equal to our expectations; but, on the other hand, we could console ourselves with the belief that our determination of the peculiar physical conditions of this great inland sea, and in particular our elucidation of the mystery of the Gibraltar current, would be fairly regarded as a success. And we venture to think that this will be admitted by such as may follow us through the discussion of general results, to which we shall presently proceed.

As Captain Calver considered himself bound not to make any unnecessary delay in returning homewards, and to take every advantage of the continuance of the fair weather and favourable breeze which we enjoyed during nearly the whole remainder of our voyage, we were reluctantly compelled to give up the idea of prosecuting any further researches in the Deep Sea; and devoted ourselves to the examination of the specimens previously collected, and to the correlation of our temperature and other results, —specially directing our attention, however, to the surface-temperature of the *embouchure* of the Strait, with the view of ascertaining whether a sudden *fall* would be observable on quitting it, corresponding to the *rise* which had been noticed on the outward voyage on entering it. This change proved to be very decided. As we kept along the southern coast of Portugal towards Cape St. Vincent, the surface-temperature averaged 73°·5. At 6 P.M. we were turning the corner of the Cape, and found the surface-temperature 72°·5. And at 8 P.M., when we were fairly in the Atlantic, we found that the surface-temperature had fallen to 69°, thus showing a difference of 4°·5. On the following day, when we were off Lisbon, the surface-temperature was 69°·5; and it gradually diminished as we proceeded northwards from that point. Although the season of the year led us to expect a rough passage across the Bay of Biscay, the weather continued remarkably fine until we reached the "Chops of the Channel," where we fell in with rather a fresh breeze; this did not interfere, however, with our anchoring at Cowes on the afternoon of the next day (October 8th), after an absence of just two months, during which a greater number of most important public events had occurred than had ever before been crowded within so short a period.

General Oceanic Circulation.—The difference as to level and density between two bodies of sea-water, which produces the vertical circulation in the Strait of Gibraltar and the Baltic Sound, may be brought about otherwise than by the excess of evaporation which maintains it in the one case, or by the continual dilution with fresh water which maintains it in the other. It may be easily shown that a constant and decided *difference of temperature* must have exactly the same effect. Let the Mediterranean basin be supposed to be filled with water of the same density as that of the Atlantic and up to the same level; and to be then cooled down below the freezing-point of fresh water by the withdrawal of solar heat, whilst the surface of the Atlantic continues to be heated as at present by the almost tropical sunshine of the Gibraltar summer. The cooling of the Mediterranean column, reducing its bulk without any diminution of weight, would at the same time lower its level and increase its density. An in-draught of Atlantic water must take place through the Strait to restore that level; but this in-draught would augment the weight of the column, giving it an excess above that of the column at the other end of the Strait; and to restore the equilibrium a portion of its deeper water must be forced out as an under-current towards the Atlantic, thus again reducing the surface-level of the Mediterranean. Now, so long as the warm Atlantic water which comes in to maintain that level is in its turn subjected to the same cooling, with consequent lowering of level and increase of density, so long would the vertical pressures of the two columns, which would be speedily restored to equilibrium if both basins were subjected to the same heat or the same cold, remain in a constant state of inequality; and so long, therefore, must this vertical circulation continue.

Now, the case thus put hypothetically has a real existence. For the Mediterranean, cooled down by the withdrawal of solar heat, let us substitute the Polar basin, and for the Atlantic, the Equatorial Ocean. The antagonistic conditions of temperature being constantly sustained, a constant interchange between polar and equatorial waters through the seas of the Temperate Zone must be the result. The reduction in the temperature of the Polar column must diminish its height whilst augmenting its density; and thus a flow of the upper stratum of equatorial water must take place towards the poles to maintain the level thus lowered. But when the column has been thus restored to an equality of height, it will possess such an excess of weight that its downward pressure must force out a portion of its deeper water; and thus an underflow of ice-cold water will be occasioned from the polar towards the equatorial areas.

The agency of polar cold will be exerted, not merely in reducing the bulk of the water exposed to it, and thereby at the same time *lowering its level* and *increasing its density*, but also in imparting a *downward movement* to each new surface-stratum as its temperature is reduced, whereby a continual in-draught will be occasioned from the warmer surface-stratum around. For the water thus newly brought under the same same cooling influence will descend in its turn; and thus, as the lowest stratum will be continually flowing off, a constant motion from above downwards will continue to take place in the entire column, so long as a fresh stratum is continually being exposed to the influence of surface-cold.

On the other hand, the agency of equatorial heat, though directly operating on only a thin film of surface-water, will gradually pump-up (so to speak) the polar water which has reached its area by creeping along the deepest parts of the intermediate oceanic basins. For since, as already shown, an in-draught of the upper stratum surrounding the polar basin must be continually going on, the place of the water thus removed must be supplied by water drawn from a still greater distance; and thus the movement will be propagated backwards, until it affects the upper stratum of the equatorial basin itself, which will flow off pole-wards, bearing with it a large measure of heat. The cold and dense polar water, as it flows in at the bottom of the equatorial column, will not directly take the place of that which has been draughted off from the surface; but this place will be filled by the rising of the whole superincumbent column, which, being warmer, is also lighter than the cold stratum beneath. Every new arrival from the poles will take its place below that which precedes it, since its temperature will have been less affected by contact with the warmer water above it. In this way an *ascending movement* will be imparted to the whole equatorial column, and in due course every portion of it will come under the influence of the surface-heat of the sun. This heat will of course raise the level of the equatorial column, without augmenting its absolute weight; and will thus add to the tendency of its surface-stratum to flow towards the lowered level of the polar area. But as the *super*-heating extends but a short way down, and as the temperature of the water beneath, down to the "stratum of intermixture," is very moderate, whilst the water below that stratum is almost as cold as that of the polar basin, it is evidently in the latter that the force which maintains this *vertical circulation* chiefly originates.

Here, then, we have a *vera causa* for a general oceanic circulation, which, being sustained only by the unequal distribution of solar heat, will be entirely independent of any peculiar distribution of land and water, provided always that this does not prevent the free communication between the polar and equatorial oceanic areas, at their depths as well as at their surface. That this agency has been so little recognised by physical geographers, we can only attribute to the prevalence of the erroneous idea of the uniform deep-water temperature of 39°, of which the temperature-observations made in our expeditions of 1868 and 1869 have shown the fallacy. Until it is clearly apprehended that *sea*-water becomes more and more dense as its temperature is reduced, and that it consequently continues to sink until it freezes, the immense motor power of polar cold cannot be apprehended. But when once this has been clearly recognised, it is seen that

the application of *cold at the surface* is, in the case of sea-water, precisely equivalent as a moving force to the application of *heat at the bottom*, the motor power of which is universally admitted, —being practically utilised, in keeping up the circulation through the hot water warming apparatus now in general use.* The movement thus maintained would not, on the hypothesis, be a rapid one, but a gradual *creeping* flow; since the absence of limit would prevent the power which sustains it from acting as an *accelerating force*, as it would do if the equatorial and polar areas were connected only by a narrow channel, like the Atlantic Ocean and the Mediterranean Sea.

That the vertical circulation here advocated on *a priori* grounds, actually takes place in any mass of salt water of which one part is exposed to surface-cold and another to surface-heat, is capable of ready experimental proof:—Let a long narrow trough with glass sides be filled with salt water; and let heat be applied at one end (the equatorial) by means of a thick bar of metal laid along the surface, with a prolongation carried over the end of the trough into the flame of a spirit-lamp; whilst cold is applied at the other (the polar) by means of a freezing-mixture contained in a metallic box made to lie upon the surface, or (more simply) by means of a piece of ice wedged in between the sides of the trough. A circulation will immediately commence in the direction indicated by the theory; as may be readily shown by introducing some *blue* colouring liquid at the polar surface, and some *red* liquid at the equatorial surface. The blue liquid, as it is cooled, at once descends to the bottom, then travels slowly along until it reaches the equatorial end of the trough, then gradually rises towards the heated bar, and thence creeps along the surface back to the polar end. The red liquid first creeps along the surface towards the polar end; and then travels through exactly the same course as the blue had previously done.†

That such a vertical circulation really takes place in oceanic water, and that its influence in moderating the excessive cold of the polar areas and the excessive heat of the equatorial region is far more important than that of any surface-currents, seems to us a legitimate deduction from the facts stated in the Report of the "Porcupine" Expedition for 1869. For, on the one hand, it was shown that there is a general diffusion of an almost glacial temperature on the bottom of the deep ocean-basins, at depths exceeding 1000 fathoms, occupied by polar water, more or less diluted by admixture according to the length of the course it has had to travel; whilst between this stratum and that other stratum of warmer water which (on the hypothesis) is slowly moving pole-wards, there is a "stratum of intermixture," in which there is such a rapid change of temperature as might be expected from the relation of the upper and lower masses of water. This "stratum of intermixture" showed itself in a most marked manner in the Atlantic temperature-observations of the present expedition; the descent of the thermometer, which had been very slow with increase of depth between 100 and 800 fathoms, becoming suddenly augmented in rate; so that between 800 and 1000 fathoms it fell nine degrees, namely, from 49°·3 to 40°·3.

On the other hand, it was shown in the previous report that there is evidence of the slow pole-ward movement of a great upper stratum of oceanic water, carrying with it a warm temperature; which movement cannot be attributed to any such local influences as those which produce the Gulf-stream or any other currents put in motion by *surface*-action. Of such a movement, it was contended, we have a marked example in that north-easterly flow which conveys the warmth of southern latitudes to the west of Ireland and Scotland, the Orkney, Shetland, and Faroe islands, Iceland, Spitzbergen, and the polar basin generally. This flow, of whose existence conclusive evidence is derived from observations of the temperature of these regions, is commonly regarded as a prolongation of the Gulf-stream; and this view is maintained not only by Dr. Petermann,* who has recently collected and digested these observations with the greatest care, but also by Prof. Wyville Thomson,† as well as by Mr. Croll.‡ Having elsewhere fully stated our objections to this doctrine, and discussed the validity of the arguments adduced in support of it, § we shall here only record the conclusions which a careful examination of the present state of our knowledge of the subject has led us to form:—

I. That there is no evidence, either from the surface-temperature of the sea, or from the temperature of sea-board stations along the western coast of Southern Europe, that the climate of that region is ameliorated by a flow of ocean-water having a temperature higher than that of the latitude: the surface-temperature of the Mediterranean Sea, which is virtually excluded from all oceanic circulation, being higher than that of the eastern margin of the Atlantic in corresponding latitudes; and the climate of sea-board stations on the Mediterranean being warmer than that of stations corresponding to them in latitude on the Atlantic coast —and this not merely in summer, but also in winter. This oceanic region may therefore be designated the *neutral area*.

II. That the evidence of climatic amelioration increases in proportion as we pass northwards from the *neutral area*; becoming very decided at the Orkney, Shetland, and Faroe islands. But that, as was shown by the *Porcupine* temperature-soundings of 1869, the flow of warm water which produces this amelioration extends to a depth of at least 700 fathoms.

III. That this deep stratum of water can be shown, by the correspondence in the rate of its diminution of temperature with depth, to be derived from the neutral area to the south-west; where, as is shown by the *Porcupine* temperature-soundings of 1870, it is separated by a distinct "stratum of intermixture" from the deeper stratum that carries polar waters towards the equator.

IV. That the slow north-easterly movement of such a mass of water cannot, on any known hydrodynamical principles, be attributed to propulsive power derived from the Gulf-stream; the last distinctly traced edge of which is reduced to a stratum certainly *not exceeding* 50 fathoms in depth, and not improbably less.

V. That on the other hand, this slow pole-ward movement of the upper layer of the North Atlantic, down to the "stratum of intermixture," is exactly what might be expected to take place as the complement of the flow of glacial water from the polar to the equatorial area; the two movements constituting a general *vertical*-oceanic circulation.

VI. That there is a strong probability that the quantity of water discharged by the Gulf-stream has been greatly over-estimated, in consequence of the rate of the surface-current having been assumed as the rate of movement through the whole sectional area, which is contrary to all analogy; whilst there is also a strong probability that there is a *reverse* undercurrent of *cold* water through the narrows, derived from the polar current, that is distinctly traceable nearly to its mouth. The upper stratum of this southerly current comes to the surface between the Gulf-stream and the coast of the United States; whilst its deeper and colder stratum underlies the Gulf-stream itself. ||

VII. That there is a strong probability that the quantity of heat carried off by the water of the Gulf-stream has been greatly over-estimated; the temperature-soundings taken during the cruise of the *Porcupine* in the Mediterranean having shown that the very high temperature of the surface extends but a little way down; whilst the temperature observations in the Atlantic show that the descent into a cold stratum beneath may be very rapid. Hence the average of 65° assumed by Mr. Croll on the basis of observations made at considerable intervals of depth, is altogether unreliable.

VIII. That the most recent and trustworthy observations indicate that the edge of the Gulf-stream to the north-east of the banks of Newfoundland is so thinned out and broken up by interdigitation with polar currents, that its existence *as a con-*

* The only scientific writer who has even approached what appears to us the truth on this point, is Captain Maury, who has put forward the doctrine of a general interchange between the equator and the poles, resulting from a difference of specific gravity caused *inter alia* by difference of temperature. But as Mr. Croll remarks, "although Captain Maury has expounded his views on the cause of ocean currents at great length in the various editions of his work, yet it is somewhat difficult to discover what they really are. This arises from the general confused and sometimes contradictory nature of his hydrodynamical conceptions." See Mr. Croll's Paper "On the Physical Cause of Ocean Currents," in the "Philosophical Magazine" for October, 1870.

† This experiment has been exhibited, by the kindness of Prof. Odling, at the Royal Institution, and at the Royal Geographical Society.

* Geographische Mittheilungen, 1870, p. 201.

† Lecture "On Deep-sea Climates," in NATURE, July 28, 1870.

‡ Memoir "On the Physical Cause of Ocean Currents," in "Phil. Mag., Oct. 1870.

§ Proceedings of the Royal Geographical Society, for Jan. 9, 1871.

|| That there is a slow southerly movement of Arctic water beneath the Gulf-stream, is indicated by the fact that icebergs have been seen moving southwards in direct opposition to its surface-flow; their deeply-immersed portions presenting a larger surface to the lower stratum than their upper part does to the more superficial layer, as in the case of our "current-drag." And similar evidence is afforded by the southward drift of the buoy which was attached to the Atlantic Cable of 1865, but which broke away from it, apparently carrying with it a great length of the wire rope by which it had been attached.

tinuous current beyond that region cannot be proved by observations, either of temperature or movement.

IX. That the Gulf-stream and other local currents put in motion by the trade-winds or other influences acting on the *surface* only, will have as *their* complement in a *horizontal* circulation return *surface* currents; and that the horizontal circulation of which the Atlantic Equatorial Current and the Gulf-stream constitute the first part is completed—so far as the Northern Hemisphere is concerned—partly by the direct return of one large section of the Gulf-stream into the Equatorial Current, and as to the other section, by the *superficial* polar currents which make their way southwards, the principal of them even reaching the commencement of the Gulf-stream.

In conclusion it may be added that the doctrine of a general vertical oceanic circulation is in remarkable accordance with the fact now placed beyond doubt by the concurrent evidence of a great number of observations, that whilst the density of oceanic water, which is lowest in the Polar area, progressively increases as we approach the Tropics, it again shows a decided reduction in the intertropical area. It has been thought that an explanation of this fact is to be found in the large amount of rainfall and of inflow of fresh water from great rivers in the intertropical region; but it is to be remembered that the surface evaporation also is there the most excessive, so that some more satisfactory account of the fact seems requisite. Such an explanation is afforded by the doctrine here advocated; the Polar water which flows towards the Equator along the bottom of the ocean basins, being there pumped up and brought to the surface.* And it is not a little confirmatory of the views advanced in this Report that in a recent elaborate discussion of the facts relating to the comparative density of oceanic water on different parts of the earth's surface, the doctrine of a general vertical circulation is advocated as affording the only feasible rationale of them.†

SCIENTIFIC SERIALS

THE *Zeitschrift fur Ethnologie* (1870 Heft III. and IV contains the following notices:—Orton's "Andes and the Amazon."—Waring's "Stone Monuments, Tumuli, &c.," "Manuscrit Troano," giving an account of the MS. in question, which is written in the Maya language; the reviewer calls this "surely the wildest production that ever saw the light with the sanction of an Imperial Government," though he admits that still wilder productions are published in his own country, now also under "Imperial government."—Benfey's "Gesch. de Sprachwissenschaft" is highly praised.—Burgen's "Temples of Satrunjaya," with forty-five photographs.—Hamy's "Paléontologie Humaine."

THE last part of the "Neues Jahrbuch für Mineralogie, Geologie," &c., published 1871, contains the following papers:—R. D. M. Verbeek on the Nummulites of the Borneo Rocks. with three plates illustrating new species, &c., one species, *N. Biaritzensis*, is also found in these beds, and extends through all the nummulitic formation from the Pyrenees to Borneo. He believes that this formation extends to Java and most of the islands of the East Indian Archipelago, but hitherto this formation has not been recognised.‡—Dr. R. Lincke on the Buntersandstein in Thüringen, which is the commencement of an elaborate monograph on these beds.—Dr. Alfred Stelzner on Quartz and Allied Minerals.—Adolph Pichler, Additions to the Mineralogy of the Tyrol; and, by the same author, Additions to the Palæontology of the Tyrol, and the usual mineralogical, geological, and palæontological notices.

OF the Transactions of the Natural History Society of Rhenish Prussia and Westphalia, including also the reports of the Society of Natural History and Medicine of the Lower Rhine, we have received the twenty-sixth volume, containing an account of the doings of the respective societies in the year 1869. The papers published by the first-mentioned society are well known to naturalists, and often of very great value. In the present volume we find the following:—"Contributions to the Rhenish Flora," by Dr. P. Wirtzen, including a discussion of the species of dog-roses, with the description of a so-called new species, *Rosa exilis*, a notice of *Asplenium Heufleri*, the description of a new plantain from Saarbruck, *Plantago Winteri*, a notice of the various forms of *Rubus tomentosus*, and of anomalies in other species of *Rubus*, and notices on the geographical distribution of certain plants; also, by the same author, a supplement to his manual of "The Flora of Rhenish Prussia; a paper "On the Height of the Water of the Rhine at Cologne from 1811-1867," by M. H. von Dechen; the continuation of Kaltenbach's valuable memoir on the German Phytophagous insects, in which the species feeding upon each species of plant are noticed, the plants being arranged in the alphabetical order of their botanical names, now reaching to the end of the letter S.; a contribution to the knowledge of the cryptogamous flora of the Saar district, by M. F. Winter, containing notices of Equisetaceæ, Lycopodiaceæ, and Ferns; and a paper, illustrated with three plates, "On the Fossil Echinodermata of North Germany," by Dr. C. Schlüter. In the last-mentioned paper, the author notices the described species of Jurassic and Cretaceous Echinoderms found in North Germany, and describes several new forms. The reports of the second society mentioned, which holds its meetings in Bonn, include an immense number of short notices of communications on almost all branches of science, but especially on Natural History and Chemistry; many of them are of considerable interest.

IN the March number of the *Journal of Anthropology* there is a careful anatomical description of the body of a negro by Dr. Kopermeki. Detailed measurements are added, together with the weights of the principal organs, and the diameter of more than twenty of the nerves. A remarkable feature in the case was the state of atrophy in which the supra-renal bodies were found; and if, in the absence of other fatal lesions, this may be assumed as the cause of death, there is here recorded a case of Addison's disease occurring in a negro. In the same journal is a translation of a review by Rütimeyer of Prof. Bischoff's work on the skulls of the anthropoid apes, in which both the text and the atlas of plates which accompanies it are severely criticised. Both the original pamphlet and the review have, however, lost much of the interest they possessed at the times of the publication, 1864 and 1868 respectively.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 30.—"Contributions to the History of Orcin.—No. 1. Nitro-substitution Compounds of the Orcins." By John Stenhouse, LL.D., F.R.S. The action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results. Schunck in this manner obtained a red resinous substance, which, by further treatment with the acid, was oxidised with oxalic acid; and in 1864 De Luynes found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, are resinous uncrystallisable substances. Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine powder is gradually added to strong nitric acid, cooled by a freezing mixture, it dissolves with a pale brown coloration, but without the slightest evolution of nitrous fumes. If this solution be now slowly dropped into concentrated sulphuric acid, cooled to -10° C., the mixture becomes yellow and pasty, from the formation of nitro-orcin, which is but slightly soluble in sulphuric acid. When this is poured into a considerable quantity of cold water, the nitro-body separates as a bright yellow crystalline powder, quite free from any admixture of resin. The crude nitro-orcin was collected, washed with a little cold water, and purified by one or two crystallisations from boiling water (40 parts). It was thus obtained in large yellow needles, which are readily soluble in hot water and but slightly in the cold; the addition of a strong acid precipitates almost the whole of the nitro-orcin from its cold aqueous solution. It is soluble in alcohol, very soluble in hot benzol, and crystallises out in great part on cooling; it is less soluble in ether, and but moderately so in bisulphide of carbon. It dyes the skin yellow, like picric acid, but is tasteless. It volatilises slightly at 100° C., melts at 162° C., and decomposes with slight explosion imme-

* That water of a *lower* should thus underlie water of a *higher* degree of salinity in travelling from the Pole to the Equator, is not difficult to account for, when the relative temperatures of the two strata are borne in mind.

† Densité, Salière, et Courants de l'Océan Atlantique, par Lieut. B. Savy, Annales Hydrographiques, 1868, p. 620.

‡ It is not out of place to mention here that Baron Reichtofen has quite recently found this nummulitic formation in China; it is described in *Silliman's Journal* for February 1871. It has been found also in Japan.

diately afterwards. When heated with concentrated sulphuric acid it dissolves, forming a deep yellow solution, which deposits crystals on cooling, and is immediately precipitated by water. It dissolves in hot strong nitric acid with evolution of nitrous fumes and formation of oxalic acid. Like picric acid, when treated with calcium hypochlorite it yields chloropicrin at the ordinary temperature. Its aqueous solutions are coloured dark brown by ferric chloride, and completely precipitated by lead subacetate. The analysis of the substance dried at 100° C. was made with the following results derived from three experiments:—

	Theory	I.	III.	II.	Mean.
C_7 =	84 = 32·43	32·58	32·63	32·68	32·63
H_5 =	5 = 1·93	2·06	2·18	2·03	2·09
N_3 =	42 = 16·22	..	..	..	..
O_8 =	128 = 49·42	..	..	..	..
	259 100·00				

These results correspond to the formula $C_7 H_5 (NO_2)_3 O_2$ that of trinitro-orcin. It is a powerful acid, much resembling picric acid, but distinguished from the latter by the greater solubility of its salts. I propose, therefore, to call this new substance *trinitro-orcinic acid*. The preparation and composition of a large number of compounds of the acid were then detailed.

Geological Society, March 22.—Prof. John Morris, Vice-President, in the chair. Messrs. A. R. Selwyn, Director of the Geological Survey of Canada; J. Bridges Lee, the Rev. Thomas Robert Willacy, B.A., and James Putnam Kimball, Ph.D, New York, were elected Fellows of the Society. The following communications were read: 1. "On the Passage Beds in the neighbourhood of Woolhope, Herefordshire, and on the discovery of a new species of *Eurypterus*, and some new Land-plants in them." By the Rev. P. B. Brodie, M.A., F.G.S. The author described as the "passage-beds" between the Silurian and Old Red Sandstone formations near Woolhope, a series of shales and sandstones, which at Perton attain a thickness of about 17 feet. Here the section includes, in descending order:—(1) Thin-bedded sandstones; (2) Dark brownish shales; (3) Yellow sandstone; (4) Olive shales; (5) Thin bedded-sandstone; (6) Olive shales, similar to No. 4. At some localities vegetable remains (*Lycopodites*, and perhaps *Psilophyton*) occur in the olive shales, which also contain several Crustacean fossils, including *Pterygotus Banksii* and a new species of *Eurypterus*, named by Mr. Woodward *E. Brodiei*. Upon this species, Mr. Woodward presented a note supplementary to Mr. Brodie's paper. Mr. Duncan inquired whether any metamorphoses had been recognised among the Eurypteridæ, and, if so, whether the variation in the thoracic plates mentioned by Mr. Woodward might be connected with them. Mr. Woodward, in reply, remarked on the difficulty of distinguishing even the sexes in Eurypteridæ. The thoracic plate in the fossils resembled that of *Limulus*, and the variety might be connected with sex. In some *Slimoniæ* from Lesmahago the only difference to be found was in the thoracic plate, and it had been suggested that this was due to difference of sex. He had already suggested that the small *Pterygotus* and the great *Slimonia* might be only the male and female forms of the same species. On fragmentary remains it was, however, unsafe to attempt to base species; but he thought *Eurypterus Brodiei* was a well-marked species. Rev. E. Winwood inquired whether there was any evidence as to *Euypterus* being freshwater or marine. The chairman observed that the seeds from the passage-beds did not appear to him other than those of land-plants, and had been previously described by Dr. Hooker as spore-cases of Lycopodiaceæ.—2. On the Cliff-sections of the Tertiary Beds west of Dieppe in Normandy and at Newhaven in Sussex." By Mr. William Whitaker, F.G.S. The author gave details of the sections of the Tertiary beds at the above places, and noticed the occurrence of London clay. Below this formation at Dieppe is a mass of sand, the same as that of the "Oldhaven beds" in East Kent, but here less markedly divided from the clay above; and beneath this sand come the estuarine shelly clays, &c., of the Woolwich beds. In the older accounts of the Newhaven section a much less thickness of the Tertiary beds is chronicled than may now be seen; indeed the successive descriptions end upwards with higher and higher beds, owing to the destruction of the coast and the wearing back of the cliff into higher ground, the highest point seeming to have been at last reached. Here the Oldhaven sand is absent, but the Woolwich clays are in greater force; and the ditch of the new fort shows some very irregular masses of gravel more or less wedged into those clays. Both sections show the comparatively wide extent of like conditions to those of the Woolwich beds, of West Kent. The Chairman, in inviting discussion, called attention to the existence of Tertiary beds of similar character near Epernay and Rheims, and in other parts of France. Mr. Evans remarked on the bearing which this extension of soft, yielding strata had on the excavation of the Channel. The disturbances in the sands and clays might be due to the springs having formerly, owing to the distance of the sea and the river-valley not having been excavated, stood at a higher level, and having thus softened or even washed away, the bed beneath the gravels. Mr. Pattison mentioned that in all the combes along the French coast towards Trêport there were traces of soft Tertiary beds, possibly Thanet sands. Mr. Whitaker, in a reply to a question from the Chairman, stated that, to the best of his belief, the sandstones at Dieppe were not calciferous. The sands were above the Woolwich beds, and therefore not Thanet sands.—3. "On New Tree Ferns and other Fossils from the Devonian." By Prof. J. W. Dawson, L.L.D., F.R.S., F.G.S. The author referred to the numerous species of ferns known in the Upper and Middle Devonian of America, and to the fact that he had described several large petioles as probably belonging to arborescent species, and also two trunks covered with aërial roots, viz. *Psaronius erianus* and *P. textilis*. He also referred to *Caulopteris Peachii* of Salter as the only tree-fern known in the Devonian of Europe. He then described remains of four species of tree-ferns in collections communicated to him by Dr. Newberry of New York. The first of these, *Caulopteris Lockwoodi*, was found by the Rev. Mr. Lockwood at Gilboa, the locality of the Psaronites already mentioned, in rocks of the Chemung group. It is a fragment of a well-characterised stem, with parts of five petioles attached to it, and associated with remains of the leaves. It must have been entombed in an erect position. and is not improbably the upper part of one of the species of *Psaronius* from the same locality. The second species, *Caulopteris antiqua*, Newberry, is of much larger size, but less perfectly preserved. It is a flattened stem on a slab of marine limestone from the Corniferous formation in the lower part of the Middle Devonian (Erian) of Ohio. The third species. *Protopteris peregrina*, Newberry, is from the same formation with the last, and constitutes the first instance of the occurrence of the genus to which it belongs, below the Carboniferous. The specimens show the form and arrangement of the leaf-scars, the microscopic structure of the petioles, and also the arrangement of the aërial roots covering the lower part of the stem. The fourth species is a gigantic *Rhachiopteris*, or leaf-stalk, evidently belonging to a species quite distinct from either of the above and showing its minute structure. It is no less than four inches wide at the base. In the cellular tissue of this petiole are rounded grains similar to those regarded by Corda and Carruthers, in Carboniferous and Eocene specimens, as starch-granules. In addition to these species, the paper described a new *Nœggerathia* (*N. gilboensis*), and noticed a remarkable specimen from Caithness, in the collection of Prof. Wyville Thomson, throwing light on the problematical *Lycopodites Vanuxemii* of America; also interesting specimens of *Psilophyton* and other genera seen by the writer in the collection of Mr. Peach of Edinburgh. Dr. Duncan doubted the desirability of basing generic and specific terms on imperfectly preserved and indistinct specimens, and pointed out the disagreements among botanists that had resulted from so doing. He would prefer calling fossils such as those described "cryptogamous forms from certain strata." He was doubtful also whether the supposed petrified starch was not merely orbicular silex. The chairman remarked on the four different conditions exhibited by existing tree ferns, first, with roots running down the stem: secondly, the lower portion with oval scars; these are, thirdly, farther up the stem, rhomboidal vertically; and, fourthly, higher up still, rhomboidal horizontally; so that were the plant fossil, distinct genera and species might be founded upon the different parts.

MANCHESTER

Literary and Philosophical Society, March 21.—Mr. E. W. Binney, president, in the chair. Dr. John Hopkinson was elected an ordinary member of the society. "On the Mechanical Equivalence of Heat," by the Rev. H. Highton. The following is an abstract of the arguments as given in the paper and brought out in the subsequent discussion:—1. The author apologised for having mentioned other names in connection with great discoveries which were undoubtedly due primarily to Dr. Joule, and spoke of the very great value of Dr.

Joule's experiments, even when he did not agree with the deductions drawn from them. 2. The subject is of extreme importance both for the interpretation of physical phenomena and or determining what limits are assigned by the stern laws of nature to the exercise of man's mechanical and scientific skill. 3. No doubt Dr. Joule has ascertained the heat ordinarily derived from the destruction of energy, by means of friction with various substances; but it has been assumed, *in defiance of facts*, that the numerical relations which connect heat and energy in the case of friction hold good when energy and heat produce or destroy each other by any other means. 4. In the case of friction itself, energy is not transformed simply into heat, but partly into heat and partly into another kind of energy, which is involved in the expansion of the solids or liquids acted on. 5. No doubt the coincidence between the mechanical equivalent of heat, found by Dr. Joule from friction, and that by M. Favre from working a magnetic engine, seems very striking; but (1) the value of Favre's experiment disappears on examination. It was but a single experiment, either never repeated, or never repeated with the same results; in a very delicate experiment there was only the difference of 300 units out of 18,000; and even the permanent enlargement which always takes place in magnets which are in use might account for this; and (2) numerous and long-continued experiments by M. Soret show results entirely discordant with the single one of M. Favre. 6. It seems incredible, that with the imperfectly constructed engine used by Joule and Scoresby, they should at the very first trial have succeeded in utilising two-thirds of the magnetism evolved, or capable of being evolved, by their battery; and Dr. Joule now tells us that according to his latest calculations of the mechanical equivalence of heat they utilised six-sevenths of the power of the battery. The only conclusion we can arrive at is, that the real power of the battery, and therefore of a grain of zinc, must have been much greater than he calculated. 7. For consider the disadvantages under which the engine acted: (1) the temporary and permanent magnets were never nearer than ¼ of an inch apart. Though Dr. Joule assures us this does not affect the *power* of the engine, it certainly produces a waste of zinc, as the near approach of the magnets creates counter-currents which check materially the consumption of zinc. (2) The copper wire was not tested for conductivity; a subject little thought of at that time, and it is found that a very small impurity in copper wire will very, very largely diminish the power of an electro-magnet. (3) The iron was not tested for specific capacity for magnetism, yet this is a most important point which is even now but little appreciated. It is found practically that, if two electro-magnets be made from the very same piece of iron, most carefully prepared, with the very same length of the same wire, without the slightest assignable cause, one will sometimes have three times the power of the other. Hence I conclude that the maximum energy capable of being evolved by a grain of zinc must be very much greater than that assigned to it by Dr. Joule. 7. Dr. Hopkinson's argument, in his paper lately read to this society, virtually amounted to this—that a well-constructed magnetic engine will get no more duty from a grain of zinc than an ill-constructed one; and consequently, I presume, that magnets might be weakened to any extent, and removed to ever so great a distance from one another, without necessarily affecting the efficiency of the engine. 8. Dr. Hopkinson has in his criticism strangely substituted $(a-b)$ for (b). In Joule and Scoresby's paper, the consumption of zinc is expressed not by $(a-b)$ but by (b); and consequently the duty of a grain of zinc not by $\frac{W}{a-b}$ but by $\frac{W}{b}$; and when the magnets are stronger and approach nearer to each other, even if W be not increased, (b) is diminished. 9. My argument was this, that since the accepted theory of the mechanical equivalence of heat is *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, then the doctrine of the mechanical equivalence of heat cannot be true; we might with equal justice call it a mechanical equivalence of cold. It is no reply to say that such facts are simple deductions from the laws of thermodynamics. This would only show that the laws of thermodynamics are inconsistent with the doctrine of the mechanical equivalence of heat. 10. The argument from the fire syringe I withdraw, as inconclusive. But I think my case was sufficiently established without it. 11. Joule and Scoresby in their paper incorrectly assume that if the quantities of electricity in the current at different times be represented by (a) and (b), the heat varies as a^2 to b^2. This is only true where the resistance is the same. In the case before us the working of the engine introduces a fresh element in resistance. 12. Again by assuming that $(a-b)$ represents diminution of quantity of the current, *and* the diminution in the zinc consumed, *and* the heat converted into useful work, they involve the supposition either that less zinc produced equal heat, or that heat was changed into useful work which was never produced at all, and therefore could not be absorbed. In fact, there was no *proof* that any heat was absorbed at all. 13. It is said that in electro-plating, electro-magnetic engines, worked by steam, are found more economical than batteries. This is in cases where a battery of many cells would be required; which is always wasteful, as a large number of equivalents of zinc must be consumed to deposit one equivalent of silver or other metal. 14. Besides, there is a far greater advantage in changing work into electricity, than electricity into work. In the former case all, or nearly all, the work is effective; in the latter, a very small portion of the electricity has hitherto been utilised. —Dr. Hopkinson said that most of Mr. Highton's objections to the mechanical equivalent of heat appear to arise from a mistake as to what is meant by the term. The nature of this mistake may be best seen in the case of a perfect heat engine, of which t_1 and t_0 are the absolute temperatures of the source and refrigerator. Then from every unit of heat leaving the source we obtain $\frac{t_1 - t_0}{t_1}$ J units of work. Now this a quantity variable with t_1 and t_0; it would be similar to most of Mr. Highton's arguments to infer that from a given quantity of heat a variable quantity of work could be obtained. But, of course, the case really is that of the unit of heat leaving the source, $\frac{t_0}{t_1}$ is lost in the refrigerator, whilst $\frac{t_1 - t_0}{t_1}$ disappears *as heat* and is converted into the work done, and the principle of the equivalence of heat and work asserts that J is constant. It will be seen that this is the mistake Mr. Highton makes in his paper in the *Journal of Science* (end of article 6). He seems there to imagine it stated, that the work done is equivalent to the whole heat thrown into the gas, and he fails to perceive that a certain portion is used to raise the temperature of the air or turpentine. This will make my criticism of his paper in the *Chemical News* clearer. Mr. Highton argued against the mechanical equivalent, and what I pointed out was, that the chemical energy, which was converted into mechanical effect and not used to heat the wire, was proportional to $a-b$, that therefore, in order to prove that there was no mechanical equivalent Mr. Highton must show $\frac{W}{a-b}$ is variable. I do not assert that a badly constructed engine will get as much heat from fuel as a good one, but merely that the work done and the heat, which has disappeared as heat and been converted into work, are in a constant ratio. Now as regards Mr. Highton's argument from the case of elastic wires—that the wire will be cooled when stretched follows from the two laws of thermodynamics, a proof may be seen in Tait's Thermodynamics, p. 105. Mr. Highton replies, "Quite true; but this only shows that one of the laws of thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat." Now the first law of thermodynamics asserts nothing else than that there is a mechanical equivalent, constant in all cases; whilst the second law, as usually stated, involves the first law, and involves nothing else but Carnot's axiom and the principle that in conduction heat flows from the hot to the cold body, both of which no one will doubt. Mr. Highton's reply is very similar to stating that one of Kepler's laws is inconsistent with the planets moving in ellipses. What Mr. Highton proposes as a paradox is then a necessary consequence of the principle he attacks. Though the doctrine of the mechanical equivalent of heat finds its firmest basis in the immortal experiments of Dr. Joule, the fact, that assuming it we can explain many phenomena, is a valuable supplementary proof.

EDINBURGH

Royal Physical Society, February 22.—Dr. Robert Brown, President, in the chair. "On the Glacial Epoch," by the Rev. P. A. Brodie. The author of this paper proposed three questions—(1) Is he correct in supposing that the popularly received opinion with respect to the glacial epoch regards it as a period of com

paratively limited duration, intermediate between the Tertiary and Quaternary eras, when a freezing climate prevailed contemporaneously over a great part of the globe? (2) Has he stated with sufficient distinctness the facts adduced in support of that opinion? (3) Do the arguments which he has brought forward prove the opinion to be erroneous?—"Notes on the Sea Otter (*Enhydra marina*, Flem.)," by Pym Nevins Compton.—"On the Tailless Trout of Islay," by Colin Hay and Peter M'Kenzie. Mr. Peach, before reading the communication he had been entrusted with, wished to say that a gentleman whom he met at the house of a mutual friend, mentioned the Tailless Trout of Islay, and as this excited Mr. Peach's curiosity, the gentleman procured from Messrs. Hay and M'Kenzie the bounteous supply now laid before you. The communication was kindly made in reply to questions put to them by Mr. Peach:—"The locality of the loch is about 1,000 feet above the level of the sea, and is on the estate of Mr. Finley of Elenlossit, Islay, and at its highest water is not more than an acre in extent. It is so shallow that a man could wade all through it; the bottom loose stone quartz, same as the surrounding mountains, and we think it is the most elevated piece of water on the island in which trout exist. It is named Lochna Maorichean, meaning that a species of fresh water 'limpet' or 'whelk,' is found on its shores, but we can say nothing about these, all we have gone there for was to capture some of its strange denizens; as to its other productions, there are small tracts of weeds here and there lying on the surface of the water, with soft pulpy stems; of the parasites of the fish or in the water we cannot speak.—Dr. J. A. Smith exhibited a specimen of the *Cottus Grœnlandicus* (the Greenland Bull-head), recently taken at the Firth of Forth.—Mr. David Grieve exhibited a photograph of the Queensland Cicada, or as it is popularly termed, Locust.

MONTREAL

Natural History Society, Feb. 27.—The President, Principal Dawson, F.R.S., in the chair.—The President exhibited illustrations of new facts in Fossil Botany. The following is an abstract of his remarks:—"The first point mentioned was the occurrence in the Devonian Shales of Kettle Point, Lake Huron, of beds containing immense quantities of spore-cases, probably of *Lepidodendron*. These beds are referred by the Geological Survey to the horizon of the Genessee shales of New York, and are stated to be twelve or fourteen feet in thickness, and to extend over a considerable area of country. Specimens in the collection of the Survey show that the bituminous matter which causes the combustible quality of the shale, is due entirely to the immense quantities of spore-cases present, which, under the microscope, appear as flattened discs scarcely more than one hundredth of an inch in diameter. Specimens of the trunks of *Lepidodendron Veltheimainum* and *Calamites inornatus* occur in the same beds. This is probably the oldest bed of fossil spore-cases known; but in later geological periods similar beds occur, the Tasmanite, or 'white coal' of Tasmania, which consists of spore-cases of ferns, being a notable instance. The author next referred to the discovery of specimens indicating the existence of three or four species of Tree-Ferns in the Devonian of New York and Ohio. He had received from Prof. Newberry of New York a specimen, showing the upper part of a stem with five leaf stalks attached to it. This he had named *Caulopteris Lockwoodi*. Three other specimens collected by Prof. Newberry in Ohio indicated the existence of three distinct species belonging to two genera. The two most important had been named by Prof. Newberry *Caulopteris antiqua* and *Protopteris peregrina*. They are from the Coniferous Limestone, and thus carry down tree-ferns to the bottom of the middle Devonian. One of them has the cellular structure and vascular bundles in such preservation as to show their microscopic structure, which is precisely similar to that of modern ferns."—Mr. A. R. C. Selwyn, Director of the Geological Survey of Canada, read a paper "On the Occurrence of Diamonds in New South Wales," by Mr. Norman Taylor, late of the Geological Survey of Victoria, and Professor Thompson, of the University of Sydney.

PARIS

Academy of Sciences, March 27.—The hall was pretty well filled, and the correspondence was rather heavy. Letters from the provinces and from foreign parts were numerous, as the insurgents had not taken possession of the Post Office, and communications were not stopped between Paris and the outer world. M. Faye presided over the sitting, which was as orderly as in former times. No trace of public emotion was to be seen in the hall where the scientific assembly meet. Numerous details were given of the meteor which was seen on the 17th inst. in southern France, and left behind an immense luminous track. These details were very welcome, as during the investment certain bold theorists maintained that falling stars, bolides, and meteorites, were produced by the same causes. New facts having been brought forward at the last sitting to show that atmospheric changes are produced in high altitudes and gradually manifest themselves in the vicinity of the air, M. Wilfred de Fonvielle sent a communication upon the truth and genuineness of this observation. He quoted letters he had received from M. Buys Ballot, the learned director of the observatory at Utrecht, when he was waiting for a favourable wind in order to return to Paris by an aërial expedition during the investment. And he concluded by showing that the best way for ascertaining the state of things at a high level was to try scientific ascents. The Academy appeared to be much pleased with the idea, but it is impossible for it to recommend the application of the scheme as long as order is not established in Paris. M. Delaunay and M. Sainte-Claire Deville disputed as to the meaning of the thermometric measures which had been taken during the investment of Paris at the observatory at Montsouris and in the Jardin des Plantes. The distance of the two stations is something less than a mile, and the difference in altitude is about thirty feet. This circumstance may account for the difference in the two sets of observation.

DIARY

THURSDAY, APRIL 6.

LINNEAN SOCIETY, at 8.—On the stigmas of *Proteaceæ*: G. Bentham, Pres. L. S.—On the generic nomenclature of *Lepidoptera*: G. R. Crotch.

CHEMICAL SOCIETY, at 8.—On Burnt Iron and Burnt Steel: W. Mattieu Williams.—On the formation of Sulpho Acids: Henry E. Armstrong.

SATURDAY, APRIL 8.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold.

TUESDAY, APRIL 11.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, APRIL 12.

SOCIETY OF ARTS, at 8.—On Boiled Oil and Varnishes: C. W. Vincent.

THURSDAY, APRIL 13.

MATHEMATICAL SOCIETY, at 8.

CONTENTS

PAGE

NOTICE

We beg leave to state that we decline to return rejected communications; and to this rule we can make no exception.

7 I

ils
in
ck.
ain
es,
een
eric
est
elle
of
M.
cht,
to
he
ate
The
is
as
M.
tric
aris
tes.
ile,
um-
ob-

Pres.

ttieu

—

ica-